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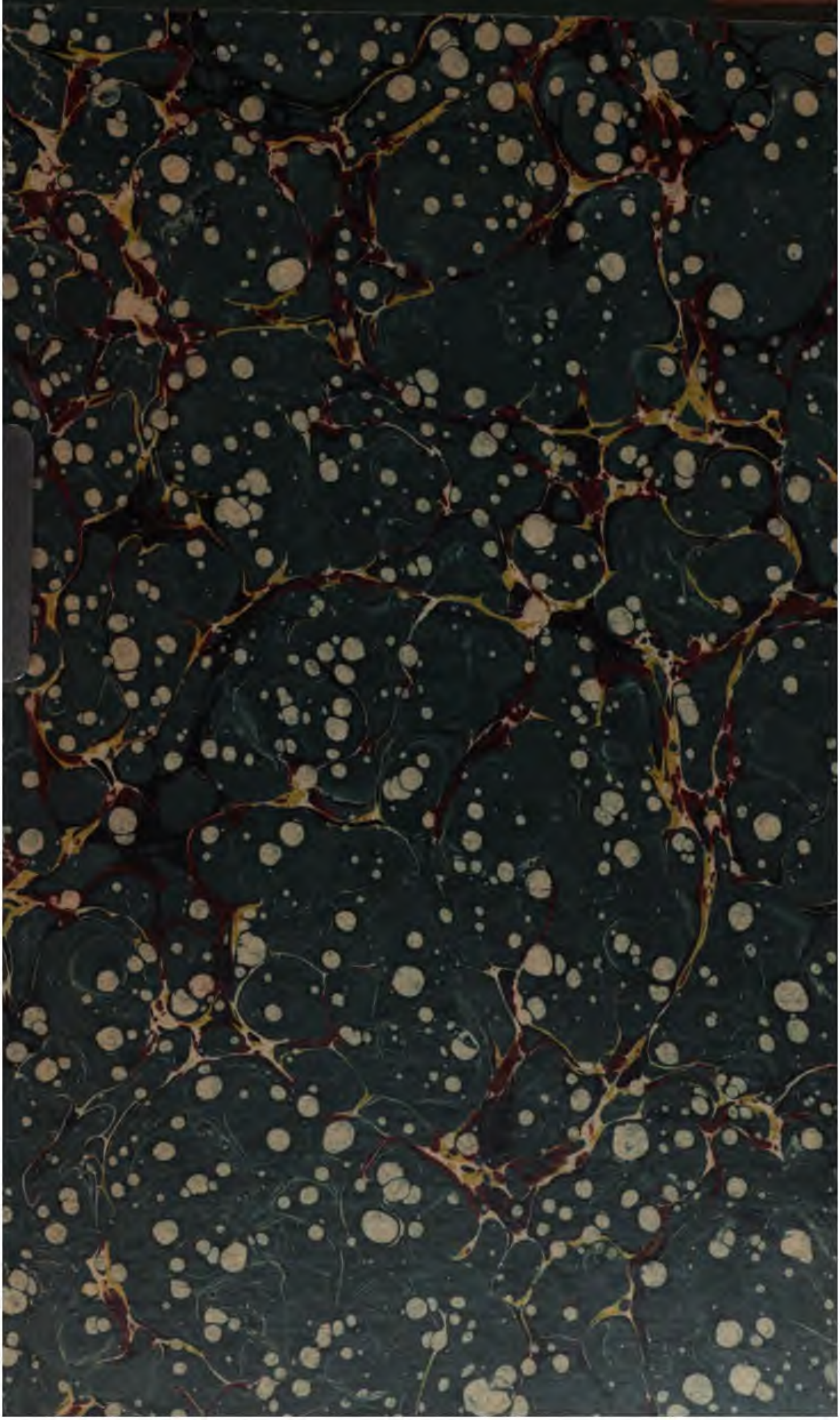

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SOME ASPECTS OF PESTILENCES AND OTHER EPIDEMICS.

By B. S. GOWEN.

I. THE BLACK DEATH.

From the time of the first recorded pestilence down to the present there have been comparatively few periods of any considerable length when pestilences or other epidemics have not prevailed in some part of the world. One of the most persistent of these is the disease now known as the Oriental or bubonic plague. It is possible that the last of "the plagues of Egypt" was this same pestilence; and there can be little doubt that the more than 50,000 Israelites and Philistines "slain by the angel of the Lord" near Bethshemesh on account of the ark (I Sam. iv, 6), as well as the 70,000 "destroyed by Jehovah" on account of David's sin (II Sam. xxiv, 15),[1] were simply victims of the pestilence which is destroying its thousands in the Orient to-day, for the symptoms, so far as they are given, are similar. It is probable, therefore, that the plague of David was identical with the pestilence which, according to Homer, broke out in the Greek camp during the Trojan war. And we may reasonably suspect that one of Job's afflictions was an attack of the bubonic plague. The plague of Athens (430 B. C.), so graphically described by Thucydides, though resembling the genuine plague in some respects, was probably not identical with it. In the description of this pestilence we get our first valuable data for a psychological study of pestilential epidemics. In the account given by Rufus of Ephesus concerning a pestilence 300 B. C. we have probably the first authentic account in profane history of the genuine bubonic plague. Procopius, an eye witness of the Justinian plague (542 A. D.), proves that scourge to be the greatest and most wide-spread pestilence known up to that time:—indeed, it has been surpassed in its sweep and destructiveness by only one in the history of the world, and that one is the Black Death.

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[1] This attribution of pestilences to the "angel of the Lord" was not peculiar to that early age; belief in their divine origin and punitive nature persisted in Europe until recently.

Though all the intervening centuries record numerous pestilences—usually five or six to the century—our attention will centre mainly in the epidemics of the later Middle Ages, and particularly in the fourteenth century, for into it were crowded more pestilences and peculiar epidemics than have ever been known at any other time. Not to mention the famine which, in the second decade of the century, strewed the roads with the dead, and caused imprisoned thieves to devour one another, nor the severe scourges of some of the more common diseases, such as measles and small-pox, there were probably twenty visits of the plague in various parts of Europe; besides, the witch mania still held sway, and the craze of the Flagellants was almost universal; the dancing mania in some form or other overran a number of European countries,—St. John's and St. Vitus's dance in central and northern Europe, and tarantism in Italy; and that most terrible (though not the most wide-spread) of all diseases, St. Anthony's fire, raged particularly in France and England. Truly this was "a century of putrid malignant affections."

The Black Death, which we shall consider first, occurred about the middle of the century, though the name which it now bears was not applied to it till about a century later. It has very appropriately been called "the greatest calamity ever visited upon mankind." Dr. Creighton, speaking of the Black Death (103a: II 133), says that although the political history of that part of the century is not unimportant, "it shrinks into insignificance in the presence of that tremendous social calamity which changed the whole face of rural England, and by transforming her agricultural system, gave a new direction to her industries, left a lasting impress on her laws, her arts, and her manners, and in a word, profoundly and permanently affected the whole future course of her political, social and economic life."

Only by a somewhat extended study of the Black Death will we be prepared to appreciate its full significance for a psychological study. Let it be understood at the outset, however, that in describing the Black Death I am but describing a severe form of the bubonic plague, for it is not a disease *sui generis*,—

a pestilence that visited the earth but once and then disappeared forever, as some have thought (1:416).

I have referred to the Black Death as the greatest calamity the human race was ever subjected to. It is true, the Justinian plague, which ushered in the Middle Ages, was called a world-pestilence, for it overran the known world of that early date, but the known world in 1348, when the Black Death, the next world-pestilence, brought the Middle Ages to a close, was a much greater world. There have been two or three scourges since that date which were less general in their sweep but even more destructive in some places. London, for example, suffered probably as much in 1603 and 1625, and perhaps even more from the "Great Plague" in 1665, when the plague as an epidemic visited England for the last time.[1] The same could be said of Milan, Marseilles, Constantinople, and other cities; but what gives the Black Death greater prominence is the fact that it was pandemic.

As to the origin of the Black Death, there are various opinions. Not a few writers have considered it a special punishment sent by the Almighty because of the wickedness of the world; others have thought that the plague was "created in the beginning," and that there has been no break in it since; still others think it unnecessary to assume that the Black Death (or the plague in any form) "was created in heaven" for the punishment of man's wickedness, or that it has existed in unbroken succession since the beginning of the world; but prefer to hold that the germs exist, and only need suitable soil or right conditions in order to develop and produce the disease. This view, it seems to me, will force itself upon one who makes a careful and exhaustive study of all the facts. The origin of the Black Death, as of other outbreaks of the plague in modern times, could no doubt be traced to natural causes in some definite place if records had been kept. Whether it originated in China,[2] as some think, or in Arabia, as others believe, I shall not discuss; but shall stop where I think absolutely authentic history stops.

We have definite proof that the plague afterwards known as the Black Death, existed at Caffa or Gaffa (now Theodosia), a

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[1] Dr. Creighton says (I. 202) that it ceased in 1666, but Brayley (Intr. to DeFoe, p. xx), says that in a milder form it persisted till 1679.

[2] Many writers, following more or less vague reports, are of opinion that the Black Death originated in China in 1333 in connection with earthquakes and famine. Dr. Creighton refers to this tendency to place the origin as far off as possible when he says that according to some epidemiologists "it is enough to have traced a virus to a remote source, to 'the roof of the earth,' or the backbone of the east wind, and there to leave it."

Genoese city in the Crimea, about the middle of the fourteenth century (30:48). In 1346 this town was attacked by the Tartars, and at some time during this siege, probably in 1347, the plague broke out among the Tartars, and carried off thousands daily. The besiegers, despairing of taking the city by force, and hoping to get possession in an indirect way, or perhaps to take revenge on the beseiged, began to use their engines of war for hurling into the city the bodies of those who had died of the pestilence (30:48). The city was abandoned; the inhabitants took to their ships and sailed westward, causing the spread of the plague in Europe. It came to Constantinople; the emperor, John Cantacuzene, following Thucydides's description of an earlier pestilence, says (40:10), "The epidemic which then raged in Northern Scythia, traversed the entire seacoast, whence it was carried all over the world. For it invaded not only Pontus, Thrace and Macedonia, but Greece, Italy, the Islands, Egypt, Libya, Judea, Syria, and almost the entire universe." A detailed study of the Black Death for the next four or five years would lead to the acceptance of such a description as this, without so much discount as would at first seem necessary.

It was early in the year 1348 that the ships which are believed to have brought the infection from the Crimean city reached Italy. Though the ships arrived safe,—some at Genoa, some at Venice, some at other ports,—not so the crews, for De'Mussi says that of a thousand sailors hardly ten were spared (30:50). At some places ships were seen without crews; drifting with the tide, touching shore here and there, and spreading the poison everywhere they touched (49:21; 40:12), just as in the country "herds were seen without a shepherd," roaming at will, and communicating the plague (30:50),—for cattle, as well as men and rats, are subject to this disease (30:46; 49:5; 24:8). It mattered little, however, whether ten or a thousand sailors reached home, for, to use Covino's expression, "one can infect the whole world." And this seems less of an exaggeration after reading such accounts as the following, by De'Mussi (30:52). "Some Genoese, who fled from the plague raging in their city, betook themselves hither to Piacenza. They rested at Bobbio, and there sold the merchandise they had brought with them. The purchaser and their host, together with all his family and many neighbors, were quickly stricken with the sickness, and died. One of these, wishing to make his will, called a notary, his confessor, and the necessary witnesses. The next day all these were buried together. So greatly did the calamity increase that nearly all the inhabitants of Bobbio soon fell a prey to the sickness, and there remained in the town only the dead." And again (p. 53): "It was the same in neighboring towns and villages. One Oberto di Sasso, who had come one day from an infected place to the church of the Friars Minor to make his will, called thither a notary, witnesses and neighbors. All these, together with others, to the number of more than sixty, died within a short space of time.—[Besides many members of the various religious orders] more than sixty dignitaries and rectors of the churches in the city and district of Piacenza died. Of nobles, too, many; of young people a vast number."

Such descriptions as this could be given of almost every city on the Italian peninsular; and not only at this date, but during many other visits of the plague, for Italy was visited sixteen times in the fourteenth century (40:31). Milan, which was the only important city to escape in 1348, was not so fortunate on other occasions, especially about two centuries later. Some of our most interesting material will be found when we take up the plague of 1630 in that city. And the description which it will be necessary to give of that scourge will make it desirable to omit details of a similar nature in other places.

It not only spread throughout the length and breadth of Italy, but found its way into France. Hardly a nook or corner of this country escaped. Li Muisis, Abbot of St. Martin's, Tournay, who was a contemporary of the events he describes, gives a vivid picture of the ravages of the plague not only in his own city, but in all France. Avignon, then the seat of Pope Clement VI, suffered more than most cities. The pope left the city for a time, and in order to make it possible for the sick to have attention, he granted plenary absolution to all who would care for the sick (40:41); otherwise it would have been difficult to get any to do this work of mercy. Marseilles was sorely afflicted, as it was a century or two later; and Paris did not escape. To show how nearly universal it was in France, one author says the plague went "from town to town, village to village, from house to house, and even from person to person." Indeed, this same statement could be made of almost all European countries, and would be no great exaggeration.

It had overrun Spain[1] before it had become so wide-spread in France. From France it took several different courses,—toward Germany, Belgium, Holland and England. After raging in England for a whole year, it found its way into Wales, Ireland and Scotland, and in 1350, into Greenland and Iceland. On the continent in this same year, it came into Switzerland apparently from different directions; it also visited

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[1] Spain suffered greatly again in 1399, when it was so depopulated that the law forbidding women to marry within a year of their husbands' death was suspended (110: I, 143).

Austria, Hungary, Poland, Norway, and Northern Europe generally, reaching Russia in 1351. Thus its sway in Europe extended from 1347 to 1351. This must be understood as a mere outline, intended to give only a general view of the course of this dread disease. It must be remembered that scarcely a town or hamlet escaped in any of these countries mentioned, and indeed, in some countries not specified, together with many islands of the sea. Creighton expresses it well when he says that not a country from China to the Atlantic escaped.

One is naturally curious to know something more in detail concerning the nature of a disease that brought such widespread desolation at one visitation,[1] to say nothing of the halt a hundred other visitations in Europe from the time of William the Conqueror till the "Great Plague" (1665), and half a score or more since that time.

In general, we may say the symptoms[2] of the Black Death were of two kinds, outward and inward. Of the outward signs, there was swelling of the gland (usually in the armpit, groin or neck) called imposthumes or buboes; dark spots on the breast or back,—known as "God's tokens," usually shortened to "tokens"—(the color of these spots gave the disease its name of "Black Death"); sometimes carbuncles in the fleshy parts; in some cases, inflammatory boils, separate or confluent. The chief inward sign was blood-spitting, or as some say, "vomiting," but while this was found wherever the plague raged, it was not found in every case—not even in all severe cases. I may mention, also, that delirium was very frequently present. Many writers mention a gangrenous inflammation of the throat, and violent pains in the chest; this no doubt was connected with the blood-spitting. There was a pestilential odor from the breath of those who vomited blood. Dr. Hodges (106 : 367) speaks of the perspiration being purple or greenish black and of the blood being colored. In the East the plague usually began with nose-bleeding, and this was a sure sign of death, as blood-spitting was in the West;—it is said that no one in England was known to live longer than a day if he began by vomiting blood (*cf.* 62 : 22). I use "vomiting" and "spitting" interchangeably, for dissection always showed the lungs to be the part affected when this symptom was present; in fact the lungs were practically consumed by a putrid inflammation. Absolute dissolution seemed to be aimed at; dissection would show more decay inside than out. In the West, the lungs were often attacked and death was near before any outward signs or buboes appeared, according to the account given by de Chauliac. Physicians sometimes opened bodies and found huge carbuncles inside. The buboes were frequently as large as a hen's egg; if they broke, or became running sores, there was hope for the patient, otherwise death occurred in a short time. The "tokens," or outward signs which almost surely indicate death, are black spots, carbuncles, or buboes, and inflammatory boils, unless, as stated above, the buboes or boils should break.

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[1] The plague returned to England five times before the end of the fourteenth century,—1361, 1368-9, 1375, 1382, 1390-1. Had not the country been so depopulated already, the scourge in 1361 would have rivaled the Black Death; it claimed more victims among children (as its name, "*pestis puerorum*," indicates) and among the higher classes than the plague in 1348,—indeed the Black Death is usually considered a disease of the lower classes, but it did not spare the great by any means; the wife and a daughter of Edward III, of England, Johanna of Navarre, Johanna of Burgundy, two brothers of the king of Sweden, Alfonso XI, of Spain, and others connected with royal families were victims of the plague.

[2] *Cf.* Gasquet: 7; Hecker: 2, ff.; Sprengel: II, 606 f.

The plague did not attack all people in the same way. Some retired at night, apparently well, and were found dead next morning; some fell into a deep sleep from which they could not be roused; some were struck suddenly and died within a few hours; others, wild with fever, could not sleep, and were consumed with a deadly thirst. It was not uncommon for persons who felt no pain to see the tokens and then be dead in a few hours. Dr. Hodges (106:262) says that men who were engaged in conversation with their friends have been known to fall suddenly into a profound and often deadly sleep. Dr. Guthrie, who was in Moscow during one visit of the plague, saw men fall as if shot (106:262); some of these, however, would recover. Dr. Alexander Russell (p. 229) confirms these reports, and Antes (p. 42) has known men to drop dead without the least warning sign.[1]

It must not be inferred that the majority died suddenly, *i. e.*, without warning; those who began with nose-bleeding or blood-spitting might live a day, but not longer. If they were attacked in some other way they might live till the third day, the day on which most deaths occurred; one who lived beyond the third day was likely to die the fifth if he died at all. One who recovered might be attacked again and again, even as many as half a dozen times, and was no more likely to die because of having had the plague before.

It can well be understood that accurate statistics could not be gathered in a scourge like this; so we are prepared for considerable difference of opinion in regard to the number of sick, the percentage of deaths, the total number of victims, and so on. Before giving the estimates of various authors on these points, it may be well to consult the reports in regard to more recent scourges where the figures can be relied upon as correct.

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[1] *Cf.* also Hecker : 6; Gasquet : 10; Sprengel : II, 606 f.; Papon : I, 172; Webster : I, 103.

Dr. McLean, who spent some time in the Levant studying the plague in 1817, says that 90 per cent. of the sick died, but he fails to give the percentage of attack. In a study of the Pali plague of 1836 (50: V, 515) Hirsch says that 30 per cent. of the entire population were attacked and that about 80 per cent. of the sick died,—giving a mortality of 24 per cent., or nearly one-fourth of the entire population. The report of the commission appointed by the English parliament 1900 and 1901 gives a similar percentage for Bombay. Sternberg, whose investigations are more recent still, gives (p. 105) the mortality of those attacked among Chinese, as 94.4 per cent. It is reasonable to conclude, therefore, that the death rate was no less in the Middle Ages when ignorance of sanitation gave the Black Death a better field than the plague can have in modern times. And as the mortality was about the same at different times and places, the death lists of some of the most important cities may give the clearest notion of what the world has suffered.

Marseilles lost in one month, 16,000; Florence, 60,000 altogether; Avignon, 60,000; Sienna, 70,000; Venice, 100,000; London, 100,000, according to Barnes and Rickmann.[1] These are not the highest estimates—Boccaccio (Intro. to Decameron) gives 100,000 for Florence, as against the 60,000 mentioned above; and 150,000 are the figures given by Gasquet (p. 37) and Anglada (p. 431) for Avignon; while Guy de Chauliac, who was a practicing physician there at the time, says (chap. V) that three-fourths of the population had the plague, and all who had it died. Moscow is said to have lost 200,000 in one visitation of the plague; during the "Great Plague," Webster says 150,000 died in Naples, or if we include the Neapolitan territories, the grand total reaches 400,000 (110:I, 190). Some places lost their 40,000 or 50,000 in a very few weeks. In many cities 1,000 or 1,500 in a day was not unusual when the plague was at its height;[2] and both Constantinople and Rome are said to have lost, at different times, as high as 10,000 in one day (49:22; 110:I, 69, 97). Different writers estimate that Italy lost half its population altogether, as a result of the Black Death (40:44; 49:26); Padua lost two-thirds, and Venice three-fourths,—the rest fled (40:26). In many places in France the pestilence carried off nine out of ten, according to Vitoduros (40:50). The number of Minorites alone who died in Italy is estimated at 30,000 (44:II, 139; 40:45). Both Walsingham and Wood have estimated England's loss at the same figures given for parts of France and Italy, *i. e.*, nine-tenths (49:25; 40:44; 1:413 f.); but others, more conservative, give it as three-fourths or even one-half.[1] At any rate, its severity in England was so great as to cause the king to prorogue parliament several times, and make a truce with France which broke off the hundred-years' war for six years (24:I, 177); also so great as to break up Oxford for a time, and seriously cripple it for many years; of the thirty thousand students claimed for the university by the chancellor, Fitzralph, before the Black Death, not more than one-third were back at work about ten years later (24:I, 189; 40:210).[2] Douglass says that three-fourths of Poland's population died. Germany's loss is estimated by Haeser and others at about 1,500,000 (49:26), 125,000 of these being friars of the "Barefooted" order (44:II 139). Many German towns of probably 200,000 were entirely bereft of inhabitants,—by death or by flight. Altogether it has been estimated that not less than 10,000 country towns and villages were left without a living soul (40:50). The king of Sweden said that scarcely a man escaped in Norway. A wilderness grew up where there had been civilization; lands were uncultivated; villages and houses were uninhabited and desolated. Not for generations did the country recover (24:I 191;

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[1] For these and other cities *cf.* Hecker: 23, 24; Sprengel: II, 606; Haeser: II, 126.

[2] It may well be imagined that the question of burial was a very serious one in times of such mortality, for the double reason that the dead were so many and the living so few. "There were scarcely enough left to bury the dead," is an expression used in all parts of the world. In fact, bodies were sometimes thrown into the street and left there for days (40:60). At Avignon the pope consecrated the Rhone so that bodies might be thrown into it (49:25). Michaud says (II, 287) that more than 400 bodies a day were counted floating down the Nile. As a rule, trenches served for graves; bodies were carried by

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the cart-load and dumped into ditches, hundreds—some say thousands —in one ditch (73 : I, 241). And we are told (73 : I, 179) that on many occasions, because of heartlessness or haste, people were buried alive.

[1] Among recent writers who believe that England lost half its population are Creighton (103a : II, 188), Corbett (103a : II, 190), Cunningham (p. 275-6) and Seebohm (pp. 93, 149); Prof. Thorold Rogers does not deny this percentage, but he believes (p. 191) that the usual estimate of 2,500,000 for England's total loss is too high, for that number, he concludes from a calculation based on the productive power of the land, is all that the country could have supported before the Black Death; Gasquet, however, having ascertained from the church rolls the number of the clergy, argues (p. 205, note) that on the basis of Rogers's estimate one man in every twenty-five was a priest,—a conclusion not easily accepted.

[2] One thing that shows how the Black Death impressed itself on the mind of the English, is the fact that documents were afterward dated from that event instead of from the reigning king, as had been customary (54 : I, 469).

40:68, 69). It is estimated that Europe altogether lost 25,000,000 during the reign of the Black Death, and China and the East, 36,000,000 (49:30). If these figures are even approximately correct it is probably true, as Hecker says (p. 30), that one-fourth[1] of the earth's inhabitants became victims of this terrible scourge.

In speaking of this universal desolation, Petrarch, who saw the ravages of the Black Death in Parma, expresses himself thus (Epist.: VIII, 7): "How will posterity believe that there has been a time when without the lightnings of heaven or the fires of earth, without wars or other visible slaughter, not this or that part of the earth, but well nigh the whole globe has remained without inhabitants? We ourselves should think we are dreaming, if we did not with our eyes, when we walk abroad, see the city in mourning with funerals; and returning to our home, find it empty, and thus know that what we lament is real." Petrarch was in a position to realize the awfulness of the pestilence, for besides seeing so much himself, his brother was in a monastery where he had to bury thirty-four of the inmates; and worst of all, perhaps, in his eyes, Laura, whom he has immortalized in his poems, died of the plague at Avignon.

It seems appropriate in this connection to quote another famous writer, Boccaccio, who, as already mentioned, was an eye-witness of the plague in Florence, in 1348. "What magnificent dwellings, what notable palaces were there depopulated to the last person! What families extinct! What riches and vast possessions left, and no known heir to inherit! What number of both sexes in the prime and vigor of youth, whom in the morning, either Galen, Hippocrates, or Æsculapius himself but would have declared in perfect health, after dining with their friends here, have supped with their departed friends in the other world" (Intr. to Decameron).

What Michaud says (II. 187), in regard to the uncertainty of life in Egypt, is equally true of many other places: "It was at the period of seed time that the plague was at its height;

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[1] Haeser (II, 137) estimates that the world lost from one-third to one-half of its population; Webster (I, 137), from one-half to three-fourths; Papon (I, 104), four-fifths. Against Hecker's estimate of 60,000,000 for the whole world, Anglada (p. 432) makes it over 75,000,000. According to Gibbon it should have been even greater, for the world's loss during a less severe scourge (the Justinian plague) was 100,000,000, he estimates (V, 503). But even if it be granted that these estimates are far too great it does not interfere with our purpose; indeed, if the figures are exaggerated it is due to the tremendous effect of the pestilence on people's minds; moreover, the belief in its great destructiveness would, even if false, produce some of the effects which we shall meet with later.

they who sowed the seed were not the same that plowed the fields; they who sowed lived not to reap the harvest."

These details concerning the plague will, it is hoped, prepare the way for a better understanding of what is to follow.

As great as were the physical effects of the plague, they were even greater, if possible, in the field of morals.[1] It is true, there were some cases of praiseworthy actions, splendid examples of devotion to duty, of heroism and self-sacrifice.

In Lübeck, while the Black Death was raging, people brought their gold and other valuables to the cloister to give to the church; finding the doors shut against all comers—to prevent, if possible, the entrance of the plague—they threw their goods over the walls (44: II, 149; 49: 24); many left their property to the church by will (97; 607). This, however, is not likely to appeal to one as an example of very great sacrifice, for with death at the door, men are not supposed to cling tenaciously to their worldly possessions. The sisters of charity in Paris astonished all by their fearless and devoted attention to the sick when more than five hundred a day were dying at the Hotel-Dieu; again and again their ranks were decimated by the plague, but volunteers were always ready to take the place of those who fell (49 : 25; 40 : 48). Monks and physicians, generally but not always, lost sight of self in ministering to others; even these deserted their post when the Black Death was at its height in Florence (40 : 13, 25, 44, 47; Boccaccio, Intr., 30 : 53).

Gasquet mentions the fact that morals even improved in some places. At Tournay, France, where very rigid laws were made so as to shut off as much of evil influence as possible, the practice of swearing diminished, and dice were made into beads or "round objects on which people told their Pater Nosters," according to Li Muisis, Bishop of St. Martin's, in that city (40:52). But he recognizes the demoralizing influence of the plague: "Instead of turning men to God, it turned them to despair in all parts of the world. Writers of all nationalities describe the same dissoluteness of manners consequent upon the epidemic." (40: Intr. xvii). He further adds this from the continuator of William of Nangis: "People were afterwards more avaricious and grasping, even when they possessed more of this world's goods than before. They were more covetous, vexing themselves with contentions, quarrels and lawsuits.

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[1] Other pestilences besides the plague, in fact, great calamities of all kinds, affect morals; but as the plague is, by far, the best single example, it—and particularly that form of it known as the Black Death—has been chosen for special consideration here, and other pestilences, famines, catastrophes, etc., will be noticed only incidentally.

Charity grew cold, wickedness with its attendant ignorance was rampant, and few were found who could or would teach children the rudiments of grammar in houses, cities or villages" (40:48). Manzoni, also, admits (p. 594) that there was good along with the bad: "There are some remarkable cases of virtue in public calamities, and in any long continued disturbance of settled habits, but an augmentation of crime is far more general. In this case [in Milan] the villains whom the pestilence spared and did not terrify, found in the common confusion and in the relaxation of public authority, a new opportunity of activity, together with new assurances of impunity." The vast majority, however, look wholly on the dark side and agree that there was an unstringing of the consciences of the survivors, which resulted in a lamentable outbreak of profligacy. In giving their opinion, I shall, in the main, let them speak for themselves.

To begin with, I shall quote from Boccaccio (Intr. to Decameron) in regard to the plague in Florence: "When the evil had become universal, all human feeling seemed to be gone; people fled from the sick to save themselves. Others shut themselves in, living aloof from others. No news of affairs outside was permitted. They spent their time in music, singing and other pastimes, but did not go to excess. Some considered eating and drinking to excess, amusements of all kinds, the indulgence of every gratification, and indifference to what was passing around them, as the best medicine, and they acted accordingly. They wandered day and night from one tavern to another, and feasted without moderation or bounds. They abandoned home and all property like men whose death-knell had already tolled. Amid the general lamentation and woe the influence and authority of every law, human and divine, vanished, and every one acted as he pleased."

Hecker (p. 49) puts it even more strongly, in these words: "Compassion, courage and the nobler feelings were found in but few, while cowardice, selfishness and ill-will, with the baser passions in their train, asserted their supremacy. In place of virtue, which had been driven from the earth, wickedness everywhere reared its rebellious standard, and succeeding generations were consigned to her baleful tyranny."

Tytler (pp. 3-4) speaks of moral degeneration, desertion of children by parents, and desertion of parents by children, of stealing and murder, and the cruel mode of preventing or punishing these (as by breaking on the wheel of torture) and also of the sick and suspects being killed,[1]—all of which indicate the tremendous effects of the plague.

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[1] Dr. Creighton (pp. 491 f.), quotes John Davies as authority for the statement that a person was drowned (by order of Sir Herbert Croft,

Assalini, who lived in the Levant during a scourge of the plague, agrees with the others when he says (p. 89) that "Our sick are separated from the rest of the family, and abandoned to their unfortunate lot, the mother even refusing to carry assistance to her own son during the agonies of death, and the husband not daring to approach the dearest object of his affections, who requests from him a drop of water, in a voice the most tender and supplicating."

Gabriel de Mussis (30: 53), who was a notary in Genoa when the Black Death reached there from Caffa,[2] describes scenes in his city exactly like those witnessed by Assalini in the Levant.

In speaking of the awful calamity that befell Muscovy, in 1603, when the plague and the famine combined claimed 500,000 victims, Noah Webster uses this language: "Parents devoured their dying children; cats, rats and every unclean thing was used to sustain life. All ties of nature and morality were disregarded; human flesh was exposed for sale in the open market. The more powerful seized their neighbors; fathers and mothers, their children; husbands, their wives, and offered them for sale" (110 : I, 171-2; *cf.* 68 : II, 187-9; 106 : 48-9; 49 : 30). These effects were probably due as much to the famine as to the plague; in general, it may be said that famine has quite as demoralizing and "denormalizing" an effect as the plague. According to Dr. Creighton (103a : II, 161) the severe famine in England in the early part of the fourteenth century not only caused people to eat the flesh of dogs and horses, but caused the inmates of jails (prisons were full of theives) to fall on and devour any new arrival who had flesh enough to tempt them. Michaud, in his "History of the Crusades," in speaking of a famine in Egypt in the thirteenth century, says (II, 187) that famine rendered every man an enemy of his fellows, and made him insensible to pity, shame and remorse. In the same connection (p. 186), he says that this terrible pair (plague and famine) not only caused mothers to devour their children, but caused people "to dispute with worms the right to the spoils of coffins." He also adds that famine and voluptuousness formed a hideous union; that all the vices of the infamous Babylon prevailed; that impure love,

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one of the council of the Marches of Wales) in order to prevent infection; Papon (I, 179) gives a case of burying alive; Gasquet (p. 138) speaks of the same; in fact, many others could be given who agree with these.

[2] De Mussis was long believed to have spent the two or three years preceding the Black Death in Caffa, and to have been in that city during the siege, but recent investigation shows no break in his records as notary in Genoa till many years after the plague.

unbounded passion for play, with all excesses of debauch, were mingled with images of death;—and this among the crusaders themselves. Modern instances are cited by Mr. J. E. Scott and Mr. Julian Hawthorne, both of whom have recently seen the combined effects of plague and famine in India. The former saw a mother and her starving infant cruelly robbed of money which he had just given her. The latter speaks of cases in which even the maternal instinct was annihilated; children were robbed of food and left to starve even by their own mothers (48 : 380).

According to Gibbon (III, 206) the effects of violent earthquakes are the same: "Instead of the mutual sympathy which might comfort and assist the distressed, they dreadfully experience the vices and passions which are released from fear of punishment; the houses are pillaged by intrepid avarice, revenge embraces the moment and selects the victim; while vengeance frequently overtakes the assassin or ravisher in the consummation of his crime."

Mezaray (quoted by Papon, I, 123) says that war needs to be classed with the plague: "At all times it was a strange thing that neither the scourge of war nor that of the pest is able to correct our nation. The dances, the pomps, the games and the tourneys continued always; the French danced, so to speak, on the graves of their parents; they seemed to testify of their rejoicing over the conflagration of their house and of the death of their friends."

Returning to the Black Death, Villani says that among the effects on the survivors of the plague in Florence may be mentioned idleness, dissolute morals, sins of gluttony, banquets in taverns, unbridled luxury, fickleness in dress, and constant change according to whim. He also says that Christian parents deserted their children in a way that might perhaps be expected of infidels and savages.

In speaking of the Justinian plague, Procopius says that whether by chance or providential design, it strictly spared the most wicked. This statement is important in that it shows the general state of morality, but it is false in so far as it intimates that the wicked who were spared constituted the wicked half of mankind before the plague, and that the good half died; *i. e.*, this view overlooks the part played by the plague in bringing about the bad moral condition spoken of. Another writer speaks of it as being "a well ascertained fact, strange though it may seem, that men are not made better by great and universal visitations of Divine Providence. It has been noticed that this is the evident result of all such scourges. From Italy to England, it seemed to rouse the worst passions of the human heart, and to dull the spiritual sense of the soul" (40:216).

Wadding, the Franciscan annalist, says: "This evil wrought great destruction to the holy houses of religion. . . . From this time on, the monastic order began to grow tepid and negligent both in that piety and learning in which they had up to that time flourished" (40:216). Dr. Cunningham (I, 275) adds that the whole social structure was disorganized. Tournay, was not alone in having unmarried men and women live together as man and wife. So flagrant did this practice become in one or two places where the moral sense was not wholly lost, special laws were passed to prevent it (40:51). Langland, whose "Piers Plowman" was intended to correct the wickedness which followed the Black Death in England, speaks of this same species of immorality in his country.

We learn from Thucydides (III, 53), that the calamity was such at Athens that men grew reckless of all law, human and divine, for no punishment was feared; no fear of God or law of man deterred a criminal. The pleasure of the moment took the place of honor and expediency. Men resolved to enjoy themselves while they lived; they were bold in their indulgence in crime; and, as we might expect, worse forms of lawlessness were introduced into Athens than had been known there before.

Papon (I, 257) uses the following language: "Who would think that in the midst of horrors, so suitable, it would seem, for extinguishing the passions, there should be found two passions which should be carried to so high a degree,—libertinism and greed. Libertinism, aroused by the frequent occasions of satisfying it, fed itself by the excess which I am ashamed to describe." The streets were often the scene of the most revolting indecency and wickedness. Indeed it is said that pedestrians sometimes found it difficult to walk the streets at night in some cities, for, aside from the obstruction offered by the corpses scattered here and there, the street was used for unholy purposes by shameless libertines.[1]

Dr. P. Russel, who studied the effects of the plague at first hand in Italy, says (I, 311) this in regard to Genoa: "Amid so many dreadful fears and terrors, amid so many fetid and putrefying bodies, amid the shrieks, the sighs and the groans of the sick, what would you have expected? That people struck with dread and horror would remain sad, modest and quiet. But not so. They sang, played on instruments, danced;—Genoa was never seen so shameless and disorderly.

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[1] I am informed by eye-witnesses that scenes like this occurred in Galveston during the flood in 1900, and that hold-ups and robbery were so common, so daring, and so openly engaged in, it became necessary for the guards to shoot down the offenders. One of Nevada's mining towns was having a similar experience when the above was written.

. . . . There was gathered a vintage for the lasciviousness. Many marriages were celebrated in the lazarettos; and many women, without shedding a tear for their husbands, immediately entered into new engagements.[1] One day in particular five marriages were performed—four of the bridegrooms being buriers of the dead, dressed in the clothes stripped from the bodies of the deceased." And in regard to Messina at a later date, he quotes an Italian authority: "It has always been observed that after every plague, those who recover are addicted in an extraordinary degree to lewdness and incontinence;—this was surprisingly visible at Messina, and was carried to such a degree of frenzy that many were known to violate the dead bodies of virgins" (*loc. cit.*).

Bulwer is not far wrong in saying that "the fiery pestilence was like a demon loosed from the abyss, to shiver into atoms all that binds the world to virtue and to law."

The following extract from Haeser (II, 144 f.). will show the opinion of a modern German writer who is the author of an exhaustive history of medicine, and who has, therefore, thoroughly canvassed the whole field, and sifted all the evidence: "A view of the moral condition is most depressing. It shows that even the greatest misfortune cannot turn the mind of men from the nothingness of the earthly, and fill it with the conception of a higher destiny. The loss in the moral realm was even greater than in the physical. Hardly had the terror of the Black Death swept over the people before all their lower passions broke forth more unbridled; and the more easily could they be satisfied through the rich inheritance of the too soon forgotten offering of the contagion. It seemed that the fearful proximity of death had only heightened their enjoyment and pleasure in life. The meanest sordidness took possession of men. . . . . Crime exalted its head above shame, for no man was there to take the laws in hand. Theft and robbery took the upper hand, and even the evident danger of death did not frighten the unbridled gain-seeker away from taking possession of the unguarded property of the dead. Even the church was guilty here and there." From the testimony of all these authors who have brought forward this array of facts in regard to the prevailing indifference, levity, frivolity and crime, accompanying pestilences, one is ready to conclude that there was an almost universal adoption of the fool's motto, "Eat, drink and be merry, for to-morrow you may die."

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[1] Papon (I, 324) says that women were married within twenty-four hours from the death of their husbands; that sometimes they married four or five times in close succession,—they seemed to have a penchant for matrimony.

The striking similarity in these different accounts is really the best proof of their trustworthiness. When simple annalists of remote cloisters, business-like or courtly chroniclers of important cities, poets and *litterateurs* like Boccaccio and Petrarch, physicians like de Chualiac and Covino, and writers who are entitled to rank as historians, all show such similarity of expression, it is only because, as Gasquet says (p. 16), "the same ideas, the same words suggest themselves involuntarily to one and all; and it is only when we come to examine the whole body of evidence that there is borne in upon the mind a realization of the nature of a calamity which, spreading everywhere, was everywhere the same in its horrors, becoming thus nothing less than a world-wide tragedy, and it is seen that even the phrases of the rhetorician can do no more than rise to the terrible reality of fact."

Any account of the Black Death which aims to present the facts which are interesting from the psychological point of view, would be incomplete if it omitted certain of the superstitions connected more or less directly with the plague. The superstition which involved the Jews is one of these, and one, too, which, because of its consequence, should be given in connection with the facts which have just been described.

It was generally believed that the plague was disseminated by means of poisonous powders or unctions, and the popular hatred of the Jews led to the accusation of that people. They were supposed to have manufactured a poison from spiders, toads, bats and owls[1]—somewhat like that the witches brewed in Macbeth—and to have put it into wells and springs. The panic which this belief caused, is said to have seized all nations; in Germany it was especially great, and resulted in the wells and springs being built over so as to make it impossible for the water to be used. Thus whole towns were forced to use river water or rain water as long as the craze lasted (49:42).

The feeling against the Jews for this supposed poisoning became very intense, and showed to what extent even sensible people can be carried by a popular wave of irrational excitement. Germany seems to have been the scene of the most diabolical treatment of these people, though there were few places where they were not regarded as outlaws, and either banished or burned (97:II, 608). All classes bound themselves by an oath to extirpate the Jews by fire and sword (49:42). In Basle, the populace forced the burgomasters and senators to bind themselves by an oath to burn all Jews then in the city, and to forbid any of that nationality from entering it for the space of two hundred years. So without sentence or trial, all the Jews in Basle were collected into a wooden building and burnt together with it (*loc. cit.*). Shortly after, the same thing happened at Freyburg, and Bennefeld in Alsace (*loc. cit.*). At Spires the Jews assembled in their own dwellings and set fire to them, and thus consumed themselves and families, preferring to die by their own hands rather than furnish a holiday for this savage multitude. The senate reserved to itself the right to search for treasure in the ruins. Many Jews were murdered in the streets and their bodies put into empty wine casks and rolled into the Rhine (49:43). When these wild passions were linked with religious fanaticism, such as we shall see later in the Flagellants, we find the most damnable practices. Some thought they could appease the wrath of an offended God by converting the Jews, and they set about the task with a zeal that knew no bounds. The Mohammedans could never have been more radical, unreasonable or cruel than these so-called Christians. At the point of the sword many Jews pretended to be converted, but later, some of them, preferring death to a living lie, slew themselves. Finally there arose a desire—a zeal, on the part of the Jews, to die as martyrs (49:44),—a fact that is interesting from our point of view. At Eslingen, the whole Jewish community shut themselves up in their synagogue, and set fire to it (44:II, 157). At Mayence, a conflict between the Jews and a body of religious fanatics (of whom we shall learn presently) resulted in the death of twelve hundred Jews, most of whom burned themselves in their own houses (49:44; 44:II, 157). The same thing took place at Munich, Constance, Ulm, Augsburg and other places (1:422; 44:II, 157). When the Jews were burned for rejecting Christianity, it was not unusual for a mother to throw her infant into the flames, rather than see it baptized, and then leap in after it (49:44; 44:II, 158).

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[1] *Cf.* Hecker, 40; Gasquet, 41; Haeser, II, 156.

Thus on both sides we see what may be called mental aberration, on a scale never seen before or since, in the history of the world. But it would be unjust to those times not to mention the fact that there was some humanitarian spirit left, especially, it would seem, among those in authority. The deputies of Strasburg voted against the punishment of the Jews, in the convention at Bennefeld. Pope Clement VI, protected the Jews at Avignon, and admonished Christians everywhere to quit such groundless persecutions,—but all in vain. Emperor Charles IV did what he dared, to protect them, but he could not prevent the Bohemian nobles from releasing themselves from their Jewish creditors; in some places the authorities even forced the Jews to return the bonds they held against others

(49 : 45; 44 : II, 159). The Duke of Austria pillaged and burned many of his own cities that had been guilty of persecuting the Jews (49 : 45),—but this was a strange way to show a humanitarian spirit; probably it furnished him a pretext to gather wealth for himself. Notwithstanding the good intentions manifested in their behalf, the Jews could have but little assurance of safety except in distant parts of Europe; thus Lithuania became for the second time a place of refuge for them (49: 46; 58: 1929).

Before leaving the Jews we should notice one thing particularly, *i. e.*, that confessions were wade by many who were innocent.[1] Similar confessions have often been extorted from witches, or even made by them voluntarily, and that, too, when escape from punishment could not be hoped for as a result of confession. The same thing is met again in lycanthrophy; and, indeed, as has been remarked, it is unusual for very many to believe for any length of time that something extraordinary is being done without some one coming forward who believes himself guilty.

Another superstition which brought serious results in an entirely different way, was the belief that a religious procession[2] would be the means of staying the plague, whereas no more efficacious means could possibly be devised for its spread. In Milan, Avignon, and practically all the cities from Italy to England, elaborate and quite spectacular processions, in which thousands took part, were of common occurrence during the Black Death, and other visits of the plague; indeed, the custom did not end with the Middle Ages,—it is practiced to-day in India, and the Philippine Islands, and probably in other places. At Oberammergau, the people resorted not to a religious procession but to a religious play,—they made a vow that if God would stay the pestilence, they would honor Him by producing this play once every ten years forever,—and so we have had the Passion Play through all the succeeding centuries.[1]

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[1] The confession of a number of Jews at Chillon, September, 1348, caused the persecution of their race to be much greater than it would probably have been. Konigshoven has preserved the original proceedings of this trial, extracts of which are given in the appendix to Hecker.

[2] The simplicity and superstition of even the higher classes as late as 1475 is well shown by the fact recorded by Rydberg (90:362) that the bishop of Lausanne issued a letter of excommunication to the insects which were infesting parts of Switzerland. The letter began thus: "Thou irrational creature, thou May-bug, thou whose kind was never inclosed in Noah's ark; in the name of the Bishop of Lausanne, by the power of the glorified Trinity through the merits of Jesus Christ, and by the obedience you owe the Holy Church, I command you to depart from all places where nourishment for man and cattle germinates and grows." They were, moreover, summoned to trial and an attorney duly appointed to defend them, the summons being solemnly read by a priest in the churchyard at Berne (*loc. cit.*).

Not only is it believed that the plague was spread and the mortality increased by superstitions that led to processions, and by superstitions such as those that prevail in India to-day, and thwart every effort of the English government to stamp out the plague; it is confidently believed that the popular state of mind had a direct effect on the death-list. In the introduction to the Decameron, Boccaccio, recognizing the principle that "pleasant thoughts are the best preventive," represents a group of persons as withdrawing to a quiet place in the country, and giving themselves up wholly to pleasure. All innocent sports and amusements were sought, but no one was permitted, under any circumstances, to bring any bad news, or mention anything of an unpleasant nature. The duty of devising new plans for passing the time pleasantly fell, each day, to a "queen" appointed for that special purpose. Story-telling was suggested by one,—and the Decameron was the result. Bulwer, following Boccaccio, has a crowd of pleasure seekers in his "Rienzi." And this was not merely a novelist's fancy—there was an historical basis for what both writers do. Pleasure parties were actually organized. About the time the Flagellants were so active companies were organized, or rather the members came together without any organization, and marched from city to city, engaging in the wildest revels. As bad as these people were, they were often taken for the Flagellants (who, as we shall see, were an avowedly religious body), so much alike were their nocturnal debauches. There were some, who, avoiding this excess, chose a middle course, and followed literally Assilini's advice (p. 93): "Banish melancholy and fear, live well, and avoid all excess; if you read, choose amusing books,—not those that treat of the plague." This calls to mind the law passed in some cities, forbidding the publication of the death list because of the depressing effect on the sick; and also the law against funeral bells, for the same reason, at Tournay, Florence and other places (40:52; 49:26). Papon believed in the power of the mind over the body, for in various places he expresses the opinion that "the solicitude, the abandon, the lack of all things produced diseases without number" (73:181, 312). Thucydides says that "people who abandoned themselves to despair threw away the one chance of life" (III, 47 f.). All this harmonizes with the remark of Assalini, Papon and others, that persons of sanguine temperament resisted well (6:31). The influence of the mind is further shown in the statement of Gasquet (p. 47) that sickness and death often result from the imagination. Furthermore, fear played an important part in augmenting the death roll,[1] so most writers on the subject believe (49:277; 40: 11, 47). Dr. P. Russel, during his stay in Italy, became thoroughly convinced that "the plague often followed the panic fear of it;" he believed that the latent infection was excited by terror (106:262). Antes (pp. 44-6) accounts for the greater mortality among the lower classes by their greater superstition. Rivinus attributes the propagation of the plague in Leipzig to fear; Willis maintains that those who fear smallpox are the first to be attacked; Chiene says it is the same with all epidemics, and Dr. Rogers agrees with this when he says that fear has much to do with the spread of all contagious diseases (52:14). Dr. Quintero, who was a hospital physician in the Canary Islands, says (pp. 14 f.) that fear made the epidemic of yellow fever much worse; he adds that his imagination got such control over him that he showed the symptoms of the disease, and had to be removed from the midst of the sufferers. A modern medical authority, Dr. C. L. Tuckey, writing on psycho-therapeutics (Wood's Medical and Surgical Monographs VIII, p. 728), says there are fear-begotten diseases of almost all kinds, among which he specifies measles, cholera, and plague. Sir F. A. Swettenham, in the preface (p. viii) to his recent book, "The Real Malay," tells of a medicine man selling "charms" to the natives to prevent the epidemic. To use his own words, "The Black Death has a way of attacking the fearful; but the Destroying Angel passes by the door of those who sleep in the happy confidence of security through the possession of a bit of magic string."[2] The comparative immunity of the Mussulmans (remarked by Dr. McLean, who lived among them) cannot be accounted for except on the basis of the influence of the mind on the body; their fatalism, although it keeps them from all precaution, frees them from fear and terror and low spirits. Personal information from Profs. G. T. Ladd and E. Washburn Hopkins, both of whom have recently studied the plague at first-hand in India, confirms this position. And not only are fatalists less likely to be attacked by the plague, the same is true of those who for any reason consider themselves immune, as in the case of the Parsees, who, according to Prof. Hopkins, were among the last to take the plague, but who died very rapidly when they did take it, for they became convinced that they were not immune, and naturally their equanimity gave way to fear and terror. In the palace at Versailles is a picture representing Napoleon visiting plague patients in a hospital in Egypt; and we are told that the great general went among the sick in order to prove that a strong will could resist the disease (111:126).

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[1] The history of the "Marienlied" is recalled in this connection. When Goldberg, Germany, was apparently bereft of all its inhabitants, one man came forth from his hiding place, and believing himself to be the only one spared began to sing the famous song. A responsive voice from a near-by house revealed a second survivor; together the two marched through the streets of the dead city singing, and were joined by others till they numbered twenty-five—all that were left alive. It was Christmas morning, and the hour was two o'clock; at the same hour every Christmas morning since that time, the people have marched through the streets of that town singing the "Marienlied."

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[1] De Foe says that in London, in 1665, twenty-three deaths were reported as caused by fear; sixteen in 1666, and seven in 1667—one death being the average for ordinary years.

[2] A similar explanation might be given of the sprinkling of blood on the lintels and door-posts during the last of the plagues of Egypt.

In brief, we have seen that melancholy and fear are the two ominous precursors of the plague; we have seen that the consensus of opinion, the universal agreement of physicians and other scientific men who have had personal knowledge, is that fear prepares the way for pestilences and epidemics of all kinds, while on the other hand, fatalism (which knows no fear) makes the Mohammedans comparatively immune.

In regard to this question the position has been taken that we know nothing of the relation of mind and body, and that facts such as those given here, although they may prove the coincidence or concomitance of great fear and great mortality, cannot prove the causal connection between mind and body, since fear is probably a *symptom* rather than a *cause*.

The objections to this criticism are (1) that it fails to disprove the theory it is aimed at; (2) that it is superficial; (3) it is self contradictory; and (4) it is otherwise unscientific. It fails to disprove, for it is certainly no simpler to assume that fear is produced by disease than to assume that disease is caused by fear. It is superficial to assume that fear in those who have the plague is caused by infection when the fear manifested by the thousands who escape the disease must be accounted for in a wholly different way. A theory that rejects the interaction of mind and body (by denying the causal power of fear) contradicts itself when it assumes that fear is a symptom or effect of the disease. Such a theory is further unscientific in violating the law of parsimony, which forbids the adoption of a more difficult explanation when a simpler one is possible. And no other theory will so simply and satisfactorily account for the facts met with in a study of pestilences, as will the theory that there is causal connection between mind and body, and that there is a causal power or force in fear.

II. THE FLAGELLANTS.

The history of the Flagellants is closely connected with the history of the plague, as will be remembered from their taking part in the persecution of the Jews, and as will be seen from what follows; but as we are going to study a class of distinctively psychical epidemics, the craze of the Flagellants may be included among them, rather than as a part of the history of the plague.

The Flagellants were people with avowedly religious intentions, who sought to avert the plague by doing penance themselves, and by bringing others to repentance. A band of this kind came together in Hungary in 1349, while the Black Death was devastating Europe; later they found their way into Switzerland, Germany, Poland and several other countries (49 : 34 ff.; 44 : II, 152 ff.; 97 : II, 607-8).

It is true, such an order had existed in the thirteenth century; it first began in Italy in 1260 and spread almost over the world, during that year and the next (49 : 36 f.; 44 : II, 152 ff.). The purpose of this was to arouse humility and penitence. While in externals this was much like the one in 1349, the latter is more interesting from our point of view, for two reasons. First, without doubt the general state of mind in Europe as a result of the terrible ravages of the Black Death, had much to do with the organization of the Flagellants, and even more in making it a psychologically interesting affair; in the second place, the reaction on the mind brought about by the actions of the Flagellants seems to have been greater, and, as Haeser says, the mental disturbance was far more significant in 1349 than in 1261 (44 : II, 152). Hence we shall confine our attention to the case in which the plague figures.

Those who first took part were mainly of the lower classes. The sincerity of most of them is not questioned; they are said to have been "seized with a deep sense of contrition, and so resolved to forsake their vices and make restitution for past offenses" (49 : 34). There were doubtless others who "joyfully availed themselves of this pretext for idleness, and were hurried along with the tide of distracting frenzy" (*loc. cit.*). Later, others from the higher classes joined the brotherhood,—even nobles and ecclesiastics and nuns, also other women of high repute, and even children. They marched in an orderly manner from city to city, singing as they went, their heads covered to the eyes, and their gaze fixed upon the ground; they wore a red cross on their breast, cap, and back. In their hands they carried scourges, on the order of the cat o'nine tails, knotted in several places, and pointed with iron (49 : 34). In front of the procession, banners and tapers were borne. Wherever they went they received the warmest welcome; bells were rung to celebrate their arrival; people flocked from all quarters to listen to their singing and witness their penance.

Their daily programme, as given by Hecker (p. 376), was as follows: "Penance was performed twice every day. Morning and evening they went abroad in pairs, singing psalms amid the ringing of bells; and when they arrived at the place of flagellation they stripped the upper part of their bodies and put off their shoes, keeping on only a linen dress reaching from the waist to the ankles. They then lay down in a large circle, in different positions, according to the nature of their crime, . . . and were then castigated, some more and some less, by their masters, who ordered them to rise, in the words of the prescribed form. Upon this, they scourged themselves, amid the singing of psalms and loud supplications for the averting of the plague."

The band that entered Strasburg was two hundred strong; but when it left it numbered over a thousand, so many recruits had it received (49:34 f.). It is said to have grown almost to the proportions of a tribe; and for several months new crowds arrived daily to join it. At Spires two hundred boys, not over twelve years old, organized themselves into a band of Flagellants, and wandered around like those who took part in the children's crusade a century before (49:35). All the inhabitants of this town were carried away with excitement. They invited the boys to their homes, and showed them every possible attention. With every pilgrimage the reputation of the juvenile Flagellants increased. The reputation of the bands of adults, on the other hand, became bad; while they engaged in religious exercises during the day, the night was given up to the wildest orgies. When this was found out they were no longer received with kindness (49:38 f.).

But whatever may be said of them toward the end, they showed every sign of seriousness at the beginning, . . . they had strict regulations that could not be violated except under penalty of the lash; each one must provide himself with a certain amount of money, in order not to be burdensome to the people, and no one must seek free entertainment or even enter a house uninvited. And all conversation even, between those of opposite sex, was forbidden. But, unfortunately, these rules were not lived up to very long; degeneracy soon crept in and all sorts of crimes were committed. When they became unscrupulous, and realized the power that numbers gave them, they became a real menace to civil and religious authority. Especially strong was the antagonism between them and the church. "They gained more credit," says Hecker (p. 38), "than the priests, from whom they so entirely withdrew themselves that they even absolved one another; besides, they everywhere took possession of the churches." They claimed to heal the sick by supernatural power, and to work miracles in other ways. In one of their meetings a letter was read which purported to have been written by an angel, urging them to penitence, and promising Christ's blessing to all who would scourge themselves and wander for thirty-four days (49:38).[1]

While the vow demanded of those joining at first, obligated them for only thirty-four days—the number mentioned in the "angel's letter,"—the plan was finally made to form a compact for thirty-four years (49:38), probably with the aim, on the part of the leaders, of forming a permanent league against the church. But the prompt and vigorous action of Emperor Charles IV, and Pope Clement prevented the carrying out of the scheme (49:38; 97: II, 608; 44:II, 157).

The tide had completely turned; the Flagellants had overreached themselves and had brought condemnation on their own heads. They were driven from every place they attempted to enter, and were hounded with bitter persecutions, as if they had been the cause of every misfortune. One of their masters was publicly burned. The Pope interdicted their public penance, and forbade the continuance of their pilgrimages, on pain of excommunication (49:39, 40).

It is evident, says Hecker, that the gloomy fanaticism which gave rise to these pilgrimages and processions of the Flagellants would infuse new poison into the already desponding minds of the people. They were carried into that "barbarous enthusiasm" which made that horrible persecution of the Jews possible.[2]

III. THE DANCING MANIA.

In the last half of the fourteenth century, which we have already found to be so prolific of strange maladies, the dancing mania, one of the most interesting epidemics in the history of the world, broke out in Germany and the Netherlands, and continued for more than two centuries (49:87; 97: II, 605 f.; 44:II, 172). It assumed different forms in different localities and different centuries, and so is not always known by this same name. For our purpose, no distinction need be made between St. Vitus's dance and St. John's dance. In the following account some of the facts relate to one and some to the other.

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[1] Haeser (II, 153) thinks this letter incident was connected with the Flagellants in 1261.

[2] Flagellants were found in Abyssinia in 1820 (49:137); and the same year a procession passed through the streets of Lisbon (69:936). Indeed, they are to be found (under the name of Penitents) among the Latin races at the present time. In a number of Mexican towns they appear quite frequently in processions, inflicting the severest punishment on themselves and on one another, even going through the form of crucifixion, but stopping just short of death (69:936). Flagellation is practiced by certain Russian sects to-day, and Stoll (p. 375 f.) says it is also to be met with among the Mohammedans.

In 1374, bands of men and women appeared in the streets of Aix-la-Chapelle, who were "united by one common delusion." Hecker (p. 87) gives the following description of their actions: "They formed circles hand in hand, and appearing to have lost all control over their senses, continued dancing, regardless of the bystanders, for hours together, in wild delirium, until at length they fell to the ground in a state of exhaustion. They then complained of extreme oppression, and groaned as if in the agonies of death, until they were swathed in clothes bound tightly round their waists, upon which they again recovered and remained free from complaint until the next attack. This practice of swathing was resorted to on account of the tympany which followed these spasmodic ravings, but the bystanders frequently relieved patients in a less artificial manner, by thumping and tramping upon the parts affected. While dancing, they neither saw nor heard, being insensible to external impressions through the senses, but were haunted by visions, their fancies conjuring up spirits whose names they shrieked out; and some of them afterward asserted that they felt as if they were immersed in a stream of blood, which caused them to leap so high.[1] Others saw the heavens open and the Saviour enthroned with the Virgin Mary, according as the religious notions of the age were strangely and variously reflected in their imaginations." Indeed, we are told that many of the dancers so completely lost their senses as not to be able to take care of themselves; in the extravagance of their actions some ran against buildings or other objects and killed themselves; others rushed into rivers and were drowned (49:130 f.).

The dancers were greatly affected by music; the magistrates in some places took advantage of this fact, and hired musicians to play for them in order that the dancers in trying to keep time to the lively tunes which were played might be exhausted more quickly; the same end was sometimes sought by hiring athletes to go in among them (49:91, 104). This extreme exhaustion of the body usually allayed the excitement of their disordered nerves, and calmed their minds.[2] The point of exhaustion was very difficult to reach in many cases; some could dance for several days, almost without ceasing, and it is reported of one woman that an entire month was required for her to find relief (49:104). The endurance of these dancers is almost incredible; that they were able to go to such extremes of violent action without serious injuries if not some fatalities, seems remarkable. But sufferers from this malady could do with impunity what would be impossible for one in the normal condition. This extraordinary power was attributed by some to the evil one, and the mania was called the "demoniacal disease" (49:88 ff.; 44:II, 175; 97:II, 605). There were some cases of permanent injury and loss of mind.

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[1] Many, knowing what convulsions they would be subject to, took the precaution to hire confidential attendants to see that they were not guilty of any impropriety (49:105).

[2] When one felt an impulse to dance, the best thing to do was to let him dance; but relief was sometimes secured in other ways. One plan was to dash cold water on the patient, or better, plunge him into a pool or a stream,—a plan practiced by Paracelsus; another plan was to scourge the patient.

Although the mania began in Germany it soon spread over the Netherlands. In Liege, Utrecht, Tongres and other cities in Belgium, bands of dancers appeared. Often they decked themselves with flowers; and usually they wore a girdle so that a bystander, by inserting a stick and twisting, might relieve tympany when the paroxysm was over (49:89).

"Wherever the dancers appeared, the people assembled in crowds to gratify their curiosity with the frightful spectacle." Many of these spectators who had come out of curiosity and for amusement, fell under the spell, and entered the ring of dancers,—reminding one of those in the "Deserted Village," who "came to scoff, but remained to pray." Besides this curious crowd, there were many parents anxiously seeking their children, and friends seeking friends who might be in the "misguided multitude" (49:89).

As in the case of the Flagellants the number of these dancers increased to an alarming extent. As a rule when they entered a town or village, they took possession of the churches and other religious houses (49:89; 97:II, 606); and again, as in the case of the Flagellants, they were received with great kindness; processions were held in their behalf; also masses were said and songs were sung, with the hope that the influence of the evil one might be removed by these means. Some of the priests even tried exorcism, for their own influence seemed to be menaced, and even their destruction seemed to be threatened by the increasing number of those "possessed," since they constantly breathed out imprecations against the priests (49:90). Furthermore, the public generally feared these fanatics, who were easily excited by the most trivial things. For example, they showed such dislike to pointed-toed shoes, which had recently come into fashion, that it was deemed advisable by the authorities to issue an ordinance that no one should wear any but shoes with square toes (49:89). But even to a greater extent were they irritated by the sight of anything red (49:89; 44:II, 175); indeed they acted like infuriated animals under these circumstances. Some were thrown into a state of excitement by the sight of any one weeping (49:90).

We find this disease raging quite severely in Metz and Cologne some time after its appearance at Aix-la-Chapelle (44:II, 172; 49:90). At Cologne there were about five hundred dancers, and at Metz more than a thousand. Hecker (p. 90) gives the following description of what occurred at these places: "Peasants left their ploughs, mechanics, their workshops, housewives, their domestic duties, to join the wild revels, and this rich commercial city became the scene of the most ruinous disorder. Secret desires were excited, and but too often found occasion for wild enjoyment; and numerous beggars, stimulated by vice and misery, availed themselves of this new complaint to gain a temporary livelihood. Girls and boys quitted their parents, and servants their masters, to amuse themselves at the dances of the possessed, and greedily imbibed the poison of mental infection. Above a hundred unmarried women were seen roving about in consecrated and unconsecrated places, and the consequences were soon perceived."

It is not strange that in times of such demoralization there were many who determined to profit by the condition of affairs. Some, seeing how well the roving bands of dancers were cared for, shrewdly conceived the idea of pretending to be possessed; and, practicing till they could imitate the dancers perfectly in their gestures and even in their convulsions, they began their wanderings, and for months they succeeded in practicing their impostures (49:91).

There is something psychologically interesting in the way the names of St. John and St. Vitus became connected with this form of the disease. As far back as the fourth century John the Baptist's day was observed. To the original customs, which were said to be strange and rude enough, relics of heathenism were superadded; bacchanalian dances became a part of the celebration (49:94). It is thought that the wild revels of St. John's dance in 1374, gave rise to the dancing mania, for the crowd that appeared at Aix-la-Chapelle continually called upon St. John's name (49:96). It may seem impossible that a festival could bring about so serious a consequence; but it must be remembered that the effect was cumulative—it held over from year to year. And besides, it is probable that other factors entered in, and that this was a culmination of a series of events, the product of a variety of causes. Neglecting for the time the reign of superstition in the preceding years and even centuries, we find certain facts which immediately preceded this breaking out, and which probably had some influence. Great parts of Germany knew no such thing as law; the barons were waging constant warfare on one another; there was no security of property, and even life was not safe; corruption of morals was the rule; the hatred and persecution of the Jews knew no bounds; besides, the masses of the Germans themselves were, as we might infer from the facts already before us, wretched and oppressed; add to this two other facts, (1) that the Rhine and the Maine had overflowed their banks and wrought immense damage, increasing the misery and distress to which the inhabitants had already been reduced; and (2) that the consciences of many were tormented with the recollection of crimes which they had committed during the prevalence of the Black Death,—tormented to such an extent that, as Hecker says (p. 96), "their despair sought relief in the intoxication of an artificial delirium,"—and then we can understand how the celebration of St. John's day could "bring to a crisis a malady that had been long impending."

Haeser, who agrees in the main with Hecker, goes a step further in trying to account for the breaking out of the mania in Germany. It is his opinion that the interdict of Pope John XXII, on the Emperor Ludwig (by which the churches were closed, by which also they were denied absolution and the benediction at death,) stirred the sensitive minds of the people, and was thus an added factor in precipitating the trouble (44: II, 174 ff.).

St. Vitus is connected in a somewhat different way (49:93 f.). He was a Sicilian youth who suffered martyrdom in the year 303. In 836, when his body was transferred to a new burying ground, he was raised to a higher rank, and not unnaturally, his altars increased in number. People had recourse to these when in distress, and besought his aid as a powerful intercessor. It is claimed that the legend which connected his name with the dancing mania was invented in the fourteenth century. The report gained currency that, just before his death, St. Vitus prayed to God that he might protect from the dancing mania all those who should "solemnize the day of his commemoration;" the legend went on to say that his prayer was accepted, and that a voice from heaven made known to Vitus the fact of this acceptance. Thus St. Vitus became the patron saint of the dancers; and his name was applied to our modern disease because of its resemblance to the dancing mania of the Middle Ages.

After the disease spent much of its force, after the regular pilgrimage of the dancers ceased, annual outbreaks occurred for some time. In these annual attacks a strong psychological effect is apparent. St. John's festival, which was held in July, and St. Vitus's day, June fifteenth, were looked forward to as great events, and naturally so, in view of the history of the preceding centuries. All minds were centred on the festival long before it came off. For weeks beforehand people, especially those who had suffered before, "felt a disquietude and restlessness which they could not overcome. They were dejected, timid and anxious, wandered about in an unsettled state, being tormented with twitching pains which seized them suddenly in different parts" (49: 105). When St. John's Day came and they danced at the altars of either St. John or St. Vitus, if for a few hours only, they received complete relief for the rest of the year,—or until the day for the festival drew near again. These occasional cases are met with as late as the end of the first quarter of the seventeenth century. The benefit that these and the dancers of the preceding centuries received, came not only from their exercise and the consequent exhaustion, but also from the simple belief that St. John and St. Vitus had power to help them.

The dancing mania in Italy assumed a somewhat different form, hence, it is necessary to speak of it separately. It was called tarantism in that country because it was supposed to be caused by the bite of the tarantula. Much of our information comes from Perrotti, who was a lecturer on medicine and other sciences at Bologna about the middle of the fifteenth century (49 :110). Later writers agree with Perrotti as to the effects of the tarantula's bite. Melancholy usually followed, and the victim's senses were deadened to a considerable degree. In almost all cases, those who had been bitten were very susceptible to the influence of music (49 :116); and would respond to a favorite tune by shouting, leaping and dancing until completely exhausted, when they would sink to the earth almost lifeless. Some, however, could not be roused from their state of melancholy, but continued downcast; their misery and anxiety showed itself in weeping. Still others "fell morbidly in love." Reports of such effects as these, from the bite of venomous serpents and insects, are to be met with at different times from the twelfth century on (49: 113); and the uneasy state of mind, the fear, the dread, that took possession of the people, together with the effects of these mental factors, bring this disease within the field of our inquiry.

There seemed to be no relief for the sufferers except that which music afforded (49:118). In a sense, music seemed to be the cause and also the cure, for one who was bitten could be roused from his state of melancholy to the wildest fits of dancing by musical instruments; and these fits could be relieved only by continuing the music till the dancer was completely exhausted. Hence it is not strange that "cities and villages alike resounded throughout the summer season with the notes of fifes, clarinets and Turkish drums." Hecker reports Alexander ab Alexandro and Matthioli both as giving very interesting examples of the effect of music. The former saw a young man seized with a violent attack of tarantism. When the drum sounded he began to dance, gradually increasing his quickness and violence until the dance became a succession of wild leaps; in the midst of this performance, the music stopped suddenly, and he fell senseless to the ground, and did not move again until the music started up, then he went through with the dance as he had done before (49 : 116 f.).

Matthioli, who was also an eye-witness, speaks of similar cases. He says that "however tortured with pain, however hopeless of relief the patients appeared as they lay stretched on the couch of sickness, at the very first sound of those melodies that made an impression on them,—they sprang up as if inspired with new life and spirit, and unmindful of their disorder, began to move in measured gestures, dancing for hours together without fatigue, until, covered with a kindly perspiration, they felt a salutary degree of lassitude, which relieved them for a time at least, perhaps even for a whole year, from their dejection and oppressive feeling of general indisposition" (49:118). In this case, as in the former, the cessation of the music before complete exhaustion, had injurious effects. So important was it to have music to alleviate the sufferings of these miserable beings, musicians were employed for this special purpose (49:118).

As with St. John's and St. Vitus's dance, tarantism was worse in summer than at other times of the year; and also, as in the case of those diseases, as summer approached and the time for the annual dance drew nigh, those affected began to feel uneasy and restless. There were regular gathering places, where they met and danced off their madness.

Many showed an abnormal fondness for water; some held glasses of water in their hand while they danced, or had large vessels standing near; in extreme cases persons are said to have been irresistibly attracted into rivers, and drowned, as in the dancing mania in Germany (49 : 120). The sufferers from tarantism abhorred certain colors, as did the dancers in the northern countries, but strangely enough they liked red, which was unbearable to the St. Vitus dancers.

Not all kinds of music were equally efficacious. Experiments showed what style was best for the various forms of the disease, and composers made their music to suit the patients. A composition intended for the dancers was called a tarantella. A number of these tarantellas have come down to us. It is thought by some that music in Italy received quite an impetus in both composition and performance, as a result of the added practice made necessary by this mania, in fact, the real beginning of the development of a musical talent among the Italians is referred by some to this period (49:116)

Many of the victims of tarantism had not been bitten by a tarantula at all, but, if they believed they had, the result was the same; and it was easy for a disordered imagination to transform a harmless insect into a tarantula. Besides, it is by no means certain that what they called a tarantula was really poisonous, but their belief that it was, and that it would lead to such terrible consequences, made possible such results as we have seen.

Although this malady was found in Abyssinia as late as 1825, it had long ceased to be epidemic in Europe, having reached its height in the seventeenth century.

Even in its decline and disappearance, it is interesting from the psychological point of view, for it was undermined by certain sceptics who considered it an imposture, and believed they had proved it to be such, by showing from experiments that the tarantula's bite would not produce the results believed to be caused by it (49; 132). When they convinced the people that the tarantula was not poisonous, the disease died out,—a fact which shows the influence of the mind in producing it. To deny the genuineness of tarantism is difficult, for an imposture that could succeed for four hundred years would certainly be remarkable.

Long before tarantism or St. Vitus's dance, *i. e.*, in 1021, we find an account of a disease which bears a strong resemblance to that of the fourteenth century (44: II, 171; 49: 98). But tradition has so colored this that we cannot be sure of the facts. The story, as it comes down to us, is to the effect that a dozen or more persons, who had disturbed public worship, had a curse pronounced upon them by which they were condemned to dance and scream for a whole year. At the end of this time, they fell into a three days' sleep, and four of them died. Another version says that, at the end of their dance, they sank into the ground up to their knees, but this could easily have grown out of the statement that they sank to their knees. According to both versions, those who survived, suffered the rest of their lives from a trembling of their limbs. How much of this is true, or whether any of it is true, is not our question, and is not of importance to us;—what is of importance is the fact that this was believed; this belief fed the flame of superstition which had already a superabundance of fuel in those years. And again, in 1278 (44: II, 172; 49: 97), a crowd of about two hundred assembled at Utrecht, and danced on the Mosel bridge while waiting, as one writer says, for the Host to be brought by; in the midst of their dance the bridge broke and the dancers were all drowned. Previously to this, however, *i. e.*, in 1237 (40: 97), more than a hundred children were seized with the dancing disease at Erfurt, and began a journey to Armstadt, dancing and jumping as they went. By the time they reached Armstadt, they were completely exhausted; some of them died and some were affected with trembling to the end of their lives. What the immediate cause of this was, I have been unable to learn, but, in all probability, this is closely allied to the "children's pilgrimage," which had taken place a short while before (in 1212), and of which religion was the inciting cause.

IV. The Children's Crusades.

Haeser quotes Hecker with approval to the effect that this pilgrimage, known as the children's crusade, is as good an example as the dancing mania of the pathological condition of those who took part (44:II, 177). A brief account of the crusade will incline one to accept this view as correct.

The date has already been given as 1212, though some place it one year later (44:II, 177). Only a short time before this Constantinople fell into the hands of the crusaders (1204), and the people were in consequence wrought up to a high pitch of excitement. This had a great effect on the minds of the youth, for the mind is so impressionable at the period of adolescence; and as a result two crusades were undertaken, one in France, the other in Germany. The one in France was probably brought to a focus by the religious processions so common at that time. Immediately after one of these, a crowd of children got together under the leadership of a shepherd boy named Stephen, who claimed to be divinely directed. He gathered round him an army of thirty thousand, mainly boys and girls, but a few older persons, and started on the way to the holy land, waving flags, and singing songs. When they reached St. Denis the king tried to turn them back, but neither advice nor command availed. They marched to the Mediterranean and thence to Marseilles. Much enthusiasm was aroused by these children, and they were everywhere kindly received.

At Marseilles they took ships for the East, but two of the ships foundered in a storm, near Sardinia, and all on board perished. In their memory the pope founded the "Church of the Innocents" near the place of the disaster. Most of those that were not drowned suffered a worse fate, for through the treachery of two merchants at Marseilles, who had offered to conduct them into Syria, they were carried to Alexandria and other places, and sold as slaves. Some of them died as martyrs (44:II, 178).

The German crowd got together at Cologne, at first without a leader, but later they chose, or rather recognized as their leader, Nicholas, a shepherd boy of only ten or twelve years, who, it seems, had done much to rouse the excitement. This army, like the other, was made up mainly of boys and girls of the shepherd classes, but, as in the other case, there were some old men; and some of the sons of the nobles joined, taking with them their paramours. Many parents were unwilling to let their children go, but could prevent it only by force. Haeser states that many who were shut in closets, tied or otherwise confined at home, actually died of disappointment (44:II, 179f.).

As they journeyed over the Alps, toward the Adriatic, they suffered many hardships; some had joined their numbers in order to steal from the inexperienced; others, in order that under the cover of piety, they might have opportunity to gratify their baser passions; some died from over-exertion, some, of starvation, and others were frozen to death. Some of them reached Italy; the pope sent cardinals to meet them and turn them back, but only a few heeded his advice. They had to break up into smaller bands, in order to get enough food. Nicholas and a band of seven thousand reached Genoa. They were not allowed to remain there more than a week because it was feared that such a band of non-producers would reduce the town to want if allowed an indefinite stay (44:II, 180). Some of the young crusaders reached Rome; some, probably a very small percentage of those who started out, got back home, but in scattered bands, barefoot, hungry, despised and mocked. The leaders had fallen under suspicion of being insincere, of being deceivers and tricksters. The moral condition was what might be expected under the circumstances; many of the girls scarcely out of childhood themselves, became mothers (44:II, 180). Thus this crusade ended in dismal failure and disgrace. But, Haeser says (II, 180f.), it shows the power of the excited imagination over the masses, and is a proof that the form of madness or delusion in any age depends on the ideas that dominate the mind.

Two centuries later there was a repetition of the children's pilgrimage, but on a smaller scale (44:II, 186f.). This time the objective point was St. Michael in Normandy. Like the others, it resulted disastrously; none of the children reached home; those that did not freeze or starve were sold as slaves. This pilgrimage is so much like the others that a detailed study of it will not add materially to our discussion.

The ordinary crusades must be passed with a word. Without doubt they were a means of spreading the plague, and were therefore responsible for whatever results the plague led to,—mental, moral, physical or economic, the last of which is far greater than is ordinarily supposed. Furthermore, there was a more direct connection between the crusades and the general tone of thought which manifested itself in the great variety of diseases peculiar to the Middle Ages. The religious motive, which led to the attempt to get possession of the tomb of Christ, is probably the most common single factor in bringing about an unbalanced state of mind; on the other hand, it would appear that some of the crusades could not have been undertaken except by those who were already unbalanced; thus the two seem reciprocally causative. Which came first is immaterial for our purpose. Grant that the crusades began under normal conditions,—we must confess that before they were at an end they stirred Europe to the very foundation. Surely it was no ordinary mental atmosphere that caused the women to despoil themselves of their most precious ornaments for the benefit of the crusaders, and the men to dispose of their hereditary domains for a small sum, or else exchange them for arms, in order to take part in the holy wars; surely under normal conditions ingots of gold could not be found in heaps where the wealthy had deposited what they no longer cared for (68:II, 21-23), nor are we prepared to see princes, under ordinary circumstances, resign all claim to their principalities, and even monarchs become indifferent to the glory of reigning over a powerful kingdom, and willing to sell their capital city, if only a purchaser could be found,—yet such was the case with Richard Cœur de Lion (98:III, 257). Such things as these are not to be expected except under an unusual order of things; and on the other hand, they would be expected to disturb greatly the mental equilibrium—if such existed—and thus prepare the soil for such a harvest of mental epidemics as we find in the Middle Ages.

V. Lycanthropy and Witchcraft.

Another disease to be mentioned in this connection is lycanthropy—a strictly mental malady. Its name indicates a belief in the ability of men to change themselves into wolves. But wolves were not the only animals into which human beings were supposed to be changed. In Africa the lion, the hyena and the jackal, in Hungary and Poland the vampire played this part (44: II, 170; 97: II, 243). Whatever animal children represented themselves to be in their play, that they believed themselves to be when the delusion came.

The history of lycanthropy is very closely interwoven with the history of magic; magicians did much to stir up and keep alive this belief; in fact, they were thought to be the teachers of those who claimed the power of changing their form. In Germany and France, belief in this power persisted even beyond the Middle Ages. It had, probably, been handed down from the days of ancient Greece, for it is known to have obtained among the Greeks in early days (44: II, 170).

In Germany, the victim of the delusion was called a *Wehr-wolf*, in France, a *loup-garou*. *Wehr-wolves* were represented as actually having a wolf-consciousness,—as being fully persuaded that they were wolves, lions, hyenas or foxes, according to the form of the delusion in any particular community. Of course, the only data possible in a case of this kind would be the action of the individual thought to have been transformed into an animal; and, it must be admitted, that often men's actions were not unlike those of a wolf. Some skulked around graveyards, and howled or barked in a way not characteristic of man in his normal condition. Some of the victims of this strange delusion further showed the propensities of wild animals by killing children (68: II, 250 ff.). Any one that went to this extent in his madness was likely to be hunted down and killed as a wild beast. In France, even as late as 1593, a hunt for men-wolves was authorized by law. The parliaments of Bordeaux, Tours, Rheims and other cities decreed that all witches and consulters with witches (by which they meant *loup-garous*), should be put to death. As a result, both at Dole and at Paris, men were brought to trial and condemned to be burned alive (68: II, 200 ff.; *cf.* 49: 108). Some of those accused, confessed, just as the Jews did during the Black Death.

If a sufficient motive could be found for men doing that for which they are sure to be hunted down as wild beasts or burned at the stake, it would be easy to believe the whole thing an imposture; but the presence of such motive is difficult to find in most cases. In Abyssinia, where this same disease, under the name of zoomorphism, existed in the early part of the nineteenth century, a sufficient motive seems to be discoverable. The potters and the blacksmiths are believed to be able to change themselves into hyenas, and are very much feared on this account; also, the church has excommunicated them because of the suspicion that there is something diabolical about it. As long as they are so regarded, their guilds are not likely to increase in number, and they thus have a monopoly in their line of work. To this end, they strive to keep themselves separate, they wear rings in their ears; and when, as sometimes happens, a hunter kills a hyena that has a similar ring in its ear, the populace has all the proof necessary that a blacksmith or a potter has changed himself into a hyena. It seems never to have occurred to them that these guilds could have caught the animal and, after inserting the ring in its ear, set it free again, in order to keep up the deception (49: 138).

As we have seen, lycanthropy and witchcraft have been closely connected historically; later, we may see that the connection is more fundamental. But in our presentation of the facts the two will be kept separate, according to the usual classification. Witchcraft will be taken in its narrow sense, as demonomania.

Witchcraft has its roots far back in the earliest times of which we have record; in sacred history and profane we have accounts of the same thing under varying forms. The witchcraft of Christian lands was that of polytheistic religions accommodating itself to a new theology. The principles of good and evil gave the Greeks their *eudaimonai* and *kakodaimonai*, the latter having their counterpart in the Roman *manes;* the satyrs, sylvans and fauns, like the Greek centaurs, were but witches considered as belonging to the "college of divinities."

It is not the witchcraft of the Old nor of the New Testament that we shall give attention to but that of modern times. In the first centuries of the Christian era witchcraft was tolerated among the French, Germans, Saxons and others; in fact, the victims were objects of pity even up to the eighth century, when the belief that the devil was present at the witches' meetings arose and aroused the church to hostility (104:II, 1370). There are records of trials of witches in Spain in the ninth century, but the burning of witches in any considerable number is not met with till the thirteenth century. From that time on to the eighteenth century (but mainly in the fifteenth and sixteenth) the number of victims is incredible, being estimated by Mackay (II, 232) as "tens of thousands," and by Rydberg (p. 361) as more than a million. One record says (68:II, 197) that in France in 1520 fires of execution blazed in nearly every town; and many of the cities of both France and Germany annually offered up several hundred each (*op. cit.*, p. 269); during the long parliament in England three hundred were condemned to death (75:290).

We now wonder how a delusion could ever have gained such hold of the popular mind as to make this wholesale destruction of life possible. But when we learn that Augustine, Luther, Melanchthon, Calvin and John Knox were firm believers in witchcraft, and that such men as Sir Thomas Brown appeared as witnesses against witches, we begin to see the extent of the craze. Not only did individuals use their influence against it, but organizations, civil and religious, attacked it. The Emperor Sigismund had the matter "scientifically" investigated in the presence of theologians, and according to Rydberg (90: XVI, 362) they came to the conclusion that it is a "positive and constant fact" that men can assume the form of animals, as in lycanthropy and witchcraft. The parliaments of most countries recognized witchcraft as a crime, and time and again made laws to punish if not prevent it; in Shakespeare's time, *i. e.*, during the reign of Elizabeth, the burning of witches was sanctioned by the English government (68:II, 149). Pope Innocent VIII issued a bull against witchcraft in 1484, and all the popes from that time till the middle of the eighteenth century endorsed his action. A knowledge of these facts will enable us to see the appropriateness of Justice Story's remarks. He speaks of witchcraft as "a belief that had the universal sanction of their own and all former ages; which counted in its train, philosophers as well as enthusiasts; which was graced by the learning of prelates as well as by the countenance of kings; which law supported by its mandates, and the purest judges felt no compunction in enforcing" (7:61).

The bull referred to above gave absolute power to inquisitors (chief of whom was Jacob Sprenger) to deal with all accused of witchcraft, and no appeal to courts or even to the pope was allowed. Sprenger wrote a book called "Malleus Malificarum" (The Witch-Hammer), giving in detail the proper plan to be used in trying witches. According to the rules laid down, trial might commence without any previous accusation. The testimony of the outlawed, the excommunicated and of reprobates who would not be allowed to appear as witnesses in an ordinary case and whose oath would not be believed on any other question, was to be accepted without question against witches. If the counsellor of the accused appeared too earnest in defence of his client he too was to be regarded as guilty of witchcraft. The person accused was put on the rack before the trial so that he might be more inclined to tell the truth. The abolition of the usual mode of trial, and the employment of torture even in the case of one under the slightest suspicion, though not accused, were advocated in the bloody code of Bodinus in France, and also in that drawn up by Henry Boguet, "The Grand Judge of Witches for the Territory of St. Claude," as he called himself. What Sprenger was to Germany, and what Bodinus and Boguet were to France, Matthew Hopkins was to England.

The offer of a reward of twenty shillings for every witch convicted gave rise to a class of professional witch finders; and the methods employed by these, and sanctioned by the courts, made the great number of executions possible.

The method of trial is one thing that makes witchcraft an interesting study from the psychological point of view. It will therefore be well to recall a few facts concerning that question.

As has been said, an inquisitor could force one to trial without accusation and without warning; he could put to torture any one he chose, in order to extort a confession. If torture failed to call forth a confession, and if witnesses were lacking, other methods were resorted to. In England, particularly, the accused was commanded to recite the Lord's prayer; if she failed to do so readily and accurately, she was adjudged guilty (68:II, 24). In some districts the one suspected was weighed against the Bible, and considered guilty if found to be lighter than it; this method seems not to have been employed extensively, for too many escaped punishment by weighing too much. Others employed the pricking test; the one on trial was pricked with pins all over the body, and if any spot insensible to pain was found, it was sufficient proof that the person was a witch. Hopkins himself preferred the swimming test recommended by King James in his "Demonologie." This required that the accused be wrapped in a sheet (after each thumb had been tied to the toe of the foot opposite) and laid on the back in a pond or river. "If they sank," says Mckay (II, 241), "their friends and relatives had the poor consolation of knowing they were innocent, but there was an end of them; if they floated—there was also an end, for they were deemed guilty of witchcraft, and burned[1] accordingly." The "Witch-finder-General," as Hopkins was called, had even a more atrocious test than this. The suspected witch was made to sit in some uneasy posture in the middle of a room for twenty-four hours, without food or drink. Guards were always present to see if Satan or any of his imps came to the witch, in the form of a fly or a moth or a bee. If any insect which entered the room succeeded in escaping before the guards could kill it, it was an imp, and the one on trial was pronounced guilty, and accordingly executed (68:II, 243). Other tests quite as ridiculous were in vogue, but illustrations need not be multiplied.

It seems that judges, parliaments, and courts, civil and ecclesiastical,[2] never realized the absurdity of their position. They accepted the testimony of witnesses who said they saw certain persons in the form of birds, cats or other animals; and they never thought to ask how these witnesses could be sure of the identity of a person in the form of a cat or a bird. The denial of one accused was disregarded; one might be honest but deluded, might be changed to an animal without knowing it, the judges held. The testimony of a husband that his wife was at home at the time she was charged with being at the witches' dance, counted for naught; he was told that he no doubt thought his wife was at home, but the devil had deceived him by substituting another or an imp in the form of his wife while she was away. This inconsistency in the way of regarding the testimony of witnesses for, and that of those against, the accused, was apparently overlooked by those engaged in the trial. And yet, as Leckey says (7:55), "the subject was examined in tens of thousands of cases, in almost every country of Europe, by tribunals which included the acutest lawyers and ecclesiastics of the age."

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[1] Regnard (pp. 37-8) says that besides burning and drowning, other methods of putting to death were employed—such as burying alive, strangling, dragging behind a cart, face down, and boiling to death.

[2] It will be remembered that the theological faculty of Paris condemned Joan of Arc to be burned as a witch.

The state of mind that made the acceptance of such evidence possible is not without interest, but the mental condition of witches themselves is of more concern to us. Of course, under the system of trials in vogue,—a system which made it possible for inquisitors to satisfy their private revenge, to gratify their cupidity or to put out of the way those whom they feared—thousands of innocent, sane and rational persons lost their lives; and this number was greatly augmented by the confessions which many of those accused made, for confessions tended to strengthen the delusion in the popular mind. These confessions themselves demand explanation,—they cannot be accounted for as deliberate lies told to escape punishment, for they always led to death. We must not forget, either, that there were witches' meetings, and these, as we shall see, gave some ground for the belief which prevailed. To quote Tamburini and Tonnini (104:II, 1369), "The grossest crimes and most barbarous cruelties were practiced at their orgies,—infants were sacrificed and their flesh, after having been boiled with toads, serpents and the like, was made into an ointment which was reputed to possess bewitching qualities. At the end of their ceremonies, great banquets were eaten at which infant's flesh was a prominent dish." Such meetings are known to have been held in Switzerland as late as the middle of the fifteenth century. This form of witchcraft is known as demonolatria, which is really devil-worship. The mental condition of witches, and especially of those who engaged in these practices, needs to be considered. It is safe to say that this condition varied all the way from simple mental depression with visions and hallucinations to the strange insanity of those who participated in these orgies or of those who hid in the woods or lurked in graveyards, killing and actually devouring people. According to Tamburini and Tonnini (104:II, 1370), an epidemic of some such character occurred in England just before, and again just after, the Black Death.

A question which has been practically neglected in the discussion of witchcraft, has quite a direct bearing on the magnitude of the delusion, and also on the poisoning mania during the plague, if not on several other epidemics of the Middle Ages. There were no asylums for the insane before the seventeenth century, so those of unsound mind were left at large (108:122 f.). There is little doubt, therefore, that they communicated their insanity to others. And no doubt this accounts, in a large measure, for the number who confessed the impossible things of which they were accused.

Turning now to the relation of lycanthropy to witchcraft, we may say that, strictly speaking, lycanthropy is a form of witchcraft; *i. e.*, witchcraft, in its broadest sense, is generic, and includes not only demonomania (witchcraft as generally understood), but theomania, lycanthropy and certain hysterical phenomena. The same general psycho-pathological condition is present in all of them. The description of melancholia given by Tamburini (104:II, 1368) will apply to the condition of the victims of witchcraft or lycanthropy. Melancholia with delusions and confused personality is clearly seen in the case of those in witchcraft who believed themselves changed into cats, birds, etc., and in the case of those in lycanthropy who believed themselves to be changed into wolves, hyenas, vampires, etc. The distinction between ordinary witchcraft and lycanthropy seems to be superficial rather than essential; in witchcraft, emphasis is not placed on the assumption of animal forms so much as on the diabolical power of the one possessed, while in lycanthropy, the change of form is the feature made prominent, although the same power is necessarily considered present.

Belief in witchcraft has not altogether died out even yet; it is probably more general in the highlands of Scotland than anywhere else. But there are no more executions; in fact, those at Salem in 1692 were among the latest so far as I have seen; the French parliament, even a century earlier, influenced, no doubt, by the the attitude of Louis XIV, had refused to condemn those accused; but trials continued in some places even in the first quarter of the nineteenth century (68 : II, 320). The delusion was so deep-rooted that with another King James and another Innocent VIII, we might find it assuming alarming proportions even now.

This remarkable delusion suggests another which, although it captivated millions of minds during its reign of a thousand years, must receive but a brief notice here. I refer to alchemy. Most noticeable among those who were entangled in its web, may be mentioned Avicenna, Albertus Magnus, Thomas Aquinas, Roger Bacon and Paracelsus. Though the philosopher's stone[1] was never discovered, though the secret of transmuting the baser metals into gold was never found out, it was a partial compensation for all the baleful influences of so powerful a superstition, for the tremendous waste of energy, time and talent, that alchemy gave us chemistry as astrology gave us astronomy.

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[1] It is interesting to notice that such men as Prof. Rutherford have concluded, from their study of radio-active substances, that the alchemists were correct in believing that substances are transmutable. So the alchemists became chemists, and chemists have become alchemists.

VI. COMMERCIAL CRAZES.

The Mississippi Scheme. The Mississippi company was organized in France, by John Law, a Scotchman, in 1717. Its purpose was to have "the exclusive right of trading to the great river Mississippi." It was a typical get-rich-quick scheme, but was on a tremendous scale, and its effects were so vast and far-reaching as to be almost beyond the possibility of belief.

To show the condition of things when the infatuation was at its height, I quote from Mackay (I, 26): "The highest and the lowest classes alike were filled with the visions of boundless wealth. There was not a person of note among the aristocracy, with the exception of the Duke of St. Simon and Marshall Villars, who was not engaged in buying and selling stock. People of every age and condition in life speculated in the rise and fall of the Mississippi bonds. The Rue de Quincampoix was the grand resort of the jobbers. . . . Houses in it, worth, in ordinary times, a thousand livres of yearly rent, yielded as much as twelve or sixteen thousand. A cobbler who had a stall in it gained about two hundred livres a day by letting it out and furnishing writing material to brokers and their clients. The story goes that a hunch-backed man who stood in the street gained considerable sums by lending his hump as a writing-desk to the eager speculators. The great concourse of persons who assembled to do business brought a still greater concourse of speculators. These again drew all the thieves and immoral characters of Paris to the spot, and constant riots and disturbances took place. At nightfall, it was often found necessary to send a troop of soldiers to clear the spot."

When fifty thousand new shares were offered, there were at least three hundred thousand applications made for them; and for weeks the eager applicants beset Law's house. Dukes, marquises and counts, with their duchesses, marchionesses and countesses, waited in the street for hours every day to know the result. Many took apartments in adjoining houses, "that they might be near where the new Plutus was diffusing his wealth" (68 : I, 25). The streets were so blocked by the waiting crowds, Law moved his office to a place that had open space enough to accommodate the crowds, and, in order to keep the streets clear a law was passed confining speculation to this place (68 : I, 28). Law became the most important personage in the realm; the ante-chamber of the regent of France was deserted by the courtiers; scholars, bishops and peers who would have considered it an insult if the regent had made them wait half an hour for an interview, thought nothing of waiting five or six hours to get to see Monsieur Law. Every conceivable scheme was devised for gaining access to him; enormous fees were paid servants by some persons merely to have their names announced. One fine lady had the coachman drive so as to overturn her carriage as she was about to meet Law, hoping that his gallantry would cause him to come to her assistance; another gave the alarm of fire in order to get to speak to him as he ran out (68 : I, 32). To use Mackay's language (I, 44), "Never was monarch more flattered than he was. All the small poets and *litterateurs* of the day poured floods of adulation upon him. According to them, he was the Saviour of the country, the tutelary divinity of France; wit was in his words, goodness in all his looks, and wisdom in all his actions. So great a crowd followed his carriage when he went abroad, that the regent sent him a troop of horse as his permanent escort to clear the streets before him."

But this state of things could not last always. Prices could not continue to rise forever. Some recovered their senses enough to think of this, then the hitherto unbounded confidence began to waver; the tide was turned; fortunes were lost by the rapid decline in stocks. Law's sincerity was questioned; he became the most cordially hated man in the kingdom,—every epithet that popular hatred could suggest was hurled at him. An attempt was made to mob him; the carriage in which he was thought to be was set upon and demolished. The royal carriage and an armed escort were put at his disposal,—for the regent had not lost confidence in him, but Law soon concluded that his safety could be assured only by flight. His property was confiscated, even contrary to a special edict that should have prevented it.

History has furnished few better examples of frenzy so well-nigh universal and of so long duration, and also of the extreme fickleness of crowds.

While this mental epidemic, this terrible delusion was raging in Paris, the plague, in its severest form, was raging in Marseilles. It is difficult to say which city was really most seriously disturbed.

The South-Sea Bubble. While France was wild over the Mississippi Scheme, England was suffering a similar delusion, usually known as the South-Sea Bubble. It promised fabulous fortunes to those who would take stock in the company organized to trade with the South-Sea countries,—certain South American states, and various islands. It is scarcely too much to say that for the greater part of a year almost all England lost its senses,—the wildest fanaticism reigned. Even Parliament had no judgment on financial questions; the encouragement it gave the South-Sea Company did much to augment the evil. The stock soon began to rise; it went to two hundred, three hundred, four hundred, five hundred, and, finally, to one thousand per cent. Mackay says (I, 83), "Exchange Alley [the Wall Street of London] was every day blocked by crowds, and Cornhill was impassable for carriages. Everybody came to purchase stock. 'Every fool aspired to be a knave.' "

The mental contagion was so powerful that the people became an easy prey to cheats, frauds and swindlers of all kinds. Dozens of other schemes of the most extravagant kind were started. Some do not believe it possible to organize a company "to make deal boards out of sawdust," yet Maitland, in his "History of London," says (68: I, 87) that such a company not only existed, but received great encouragement. There was another "for the transmutation of quicksilver into a malleable fine metal," and one for extracting silver from lead. A company, capitalized at a million dollars, to construct a wheel to be run by perpetual motion, was not the only proof of the almost universal insanity. There was a company to make square cannon balls. Permits to join a company which would be organized "sometime," for the manufacture of a new kind of sail-cloth, were sold at sixty guineas each,—the stock had to be paid for in addition to this. There was another, entitled "A company for carrying on an undertaking of great advantage, but nobody to know what it is." Mackay (I, 88) says he is following scores of credible witnesses when he states that even so palpable a swindle as this found dupes. Within five hours after the swindler opened his office for the sale of stock, a thousand shares of one hundred pounds each had been subscribed for, and partly paid; the rascal, finding himself in possession of two thousand pounds for so little outlay, left immediately for the continent, and was never heard of again.

After about eight months the South-Sea Bubble burst, because the unnatural state of mind could not last always. Prices fell rapidly; fortunes were lost in a day; many who were wealthy in the morning, were paupers at night. And as intimated elsewhere, the bitterest feeling was aroused. Parliament, which had given encouragement to the scheme before, now confiscated the property of the organizers of the company.

The Tulip Mania. It was not so strange to see the mercurial French run wild after a chimera, as to see the cold-blooded Anglo-Saxons, and especially so prudent a people as the Dutch. Yet Holland went mad over the tulip about a century before France and England had their crazes,—or more exactly, in 1634. At this date "the ordinary industry of the country was neglected, and the population, even to the lowest dregs, embarked in the tulip trade" (68:I, 141). The mania had reached such a stage in 1635 that tulips, like precious stones, were sold by weight; the standard was the *perit*, less than one grain. Many paid as much as 100,000 florins for forty bulbs. Some invested their entire fortune in the purchase of one bulb. If a man had no money, he gave what he had. One person offered twelve acres of building ground for a single root of a fine variety, the Haarlem; another gave in exchange for an Amsterdam, "4,600 florins, a carriage, two gray horses and a complete set of harness" (68:I, 142).

In view of all this, we are not suprised that a sailor who ate a tulip, believing it to be an onion, was put in prison for several months. The value set upon that particular bulb was $1,400—enough to provision a ship's crew for a whole year.

Mackay says (I, 147): "Nobles, citizens, farmers, mechanics, seamen, footmen, maid-servants, even chimney sweeps and old clothes-women, dabbled in tulips." Those who had property sold it at ruinous prices in order to become speculators.

It should be said that it was love of the flowers that led to high prices; speculation followed naturally; and when it began, foreigners caught the craze, and poured into Holland from all directions. The bulbs were publicly sold in the Stock Exchange in London, in 1636, and several years later one tulip brought seventy-five pounds.

In Holland it became necessary to enact a special code of laws for the guidance of dealers in tulips. They had their tulip-notaries and tulip-clerks who devoted all their time to tulip trade. The fever was at its height for about two years, when the people regained their senses, and found their country practically bankrupt (68:I, 147).

VII. LATER RELIGIOUS EPIDEMICS.

Interesting phenomena are to be found among religious sects even as far back as the beginning of the second century of the Christian era. The Adamites, who prayed nude, the Ophites, who worshipped the serpent, the Gnostics, with their loathsome religious rites, the Montanists, who always carried lamps and who had visions in which they saw the soul, "soft, delicate and shining," the Simmachians, who practiced self-mutilation, and the Massabiani, who, besides showing other peculiarities, always carried sacks on their backs, breaking forth into wild leaps, all possess very great interest from our point of view, but because of the limited scope of our undertaking it is considered best to concentrate on certain religious epidemics of modern times. Beginning with certain sects which arose in the seventeenth century, we shall give a brief sketch of these in order to obtain facts for a comparative study, and then take up more fully some remarkable examples of a later date.

George Fox, the founder of the Quakers, claimed to have the power of prophecy and clairvoyance, and also of miraculous healing. His mental history shows striking resemblances to that of Mohammed. He spent much time in solitude, and fasted much. Once he fell into a trance which lasted fourteen days (69:938). But it is the peculiarity of the sect which he founded to which attention is now to be given. The name given to the sect indicates this peculiarity; the "quaking" or trembling which overcame them while engaged in worship was sometimes so violent that even the house in which they sat seemed to be shaken (*cf.* Art. "Quakers," in Encyclopedia Brittanica).

The Ranters and Fifth-monarchy men who arose about the same time, *i. e.*, about the middle of the seventeenth century, were not affected so much by these convulsions, but were even worse in their wild fanaticism or mental aberration. In 1657 the Fifth-monarchy men formed a plot to kill Cromwell; and later they broke out in insurrection, claiming to have Jesus as their leader. Troops were called out against them and most of them were slain,—they would not yield because they considered themselves invincible (69 : 93).

At the end of the seventeenth century the Cevennes, or French phophets, another sect of convulsionists arose, and being driven from their own country, spread the disease in Germany, Holland and England; in the latter country it came to the attention of Charles Wesley, who considered the contortions of the body to which the victims were subject, as the work of Satan. Evidently Wesley did not suspect that later his brother's preaching would produce similar results, yet it is among his followers that some of the best examples are to be found. Indeed, Wesley's biographer seems to consider it an honor to Wesley that he could compare so favorably with Whitefield in this respect (69 : 940). And Wesley, so far from seeing anything diabolical in it, "acknowledged the finger of God." It is interesting to learn from his description, that even sceptics might be attacked while watching those affected (69 : 940 f.).

The Jumpers, who arose in Wales about 1740, are described by Wesley as presenting the same phenomena that we have found among his own followers. According to him, "they would sing the same song over and over again, thirty or forty times, till some of them worked themselves into a sort ot drunkenness or madness; then they were violently agitated and leaped up and down in all manner of postures frequently for hours together" (69 : 940).

Just about a century after the founding of the Quakers, by Fox, another similar sect arose of which the ruling spirit was "Mother" Anne Lee, whom her followers believed to be the reincarnation of Christ. She and her followers claimed the gift of prophecy, the gift of healing, and sometimes the gift of tongues. Their name also was given them because of the violent shaking which usually came upon them during their worship. Dancing was considered a part of worship, according to their own historians (69 : 941-2). After their removal to New York, near the close of the eighteenth century, many grave charges were made against them, and some of these are admitted by the Shakers themselves. Thomas Brown, who was for a time a member of the Shaker society, and who has given us an excellent history of that sect, says (p. 322) that it was their custom to shut themselves in from the world, and dance absolutely naked, men and women together; yet strange to say, he denies the rumors that the grossest immorality was practiced on these occasions.—he even says that one young woman was stripped naked and publicly whipped for manifesting improper desires. In the face of all this one is naturally curious to know what this historian is keeping back in regard to the practices of these people, for he says there are some things which modesty forbids him to speak of.

About the time the Shakers arose in England the Convulsionnaires arose in France. The grave of a greatly beloved man had been visited for years. Finally it was reported that miracles had taken place at his grave; the excitement caused was so great that some were seized with "convulsions and tetanic spams." This caused multitudes to go to the cemetery daily to witness the strange spectacle. The excitement assumed such proportions that Louis XV issued an order that the cemetery should be closed,—but laws are powerless under such circumstances. The number of victims increased to such an extent that soon there were almost a thousand Convulsionnaires.

Many of the patients, in addition to suffering from convulsions, were attacked by violent pains, which were relieved by their friends (called *secourists*), by very rough treatment, as were the dancers of the Middle ages. According to Hecker (p. 148), "the sufferers were beaten and goaded in various parts of the body with stones, hammers, clubs, swords, etc., of which treatment the defenders of this extraordinary sect relate the most astounding examples, in proof that severe pain is imperatively demanded by nature, in this disorder, as a counter-irritant.—It is stated that some Convulsionnaires have borne from six to eight thousand blows, . . . without danger." This author says further (*loc. cit.*), that "sometimes the patients bounded from the ground impelled by the convulsions, like a fish when out of water; hence it is not strange that women and girls, not wishing to appear indecent, put on gowns made like sacks, closed at the feet. . . . They showed great agility,[1] . . . and it is scarcely necessary to remark that the female sex especially, was distinguished by all kinds of leaping, and almost inconceivable contortions of the body. Some spun round on their feet with incredible rapidity, as is related of the dervishes; others ran their heads against walls, or curved their bodies like rope-dancers, so that their heels touched their shoulders."

This mania of the Convulsionnaires continued to rage for about sixty years, or until the upheavals of the French Revolution struck it a blow that weakened its force, and gradually brought it to an end. The moral condition of the community during this time is hard for us to realize. "The grossest immorality," says Hecker (p. 149), "found in the secret meetings of the believers, a sure sanctuary, and in their bewildering devotional exercises, a convenient cloak." Secret meetings are spoken of in this quotation;—in the year 1762, parliament passed a law against the "Grands Secours," or their harsh method of treatment, and this, to them, was virtually a law against their religion; or at least, a law against public meetings, for they felt that they must "cure" the suffering even if they had to have secret meetings for that purpose; and so from this time on they met in secret. Whether these extravagances continued or not after the Revolution, I have been unable to ascertain; the sect was still in existence at the end of the first quarter of the nineneenth century, but without the convulsions (49:150).

We may say that religious mania was not only epidemic, but pandemic, during the eighteenth century, for it spread throughout America as well as Europe. And as America furnishes some of the latest and best material, all further information may be drawn from our own country.

Although the epidemic was not confined to any particular section, the best example is the one known as the Kentucky Revival, which, about the end of the century, swept over several states, but centred in Kentucky. Here everything else dwindled into insignificance, and for four or five years all minds were concentrated on religion. During this time camp-meetings were in almost continuous progress, and people came in wagons forty or fifty miles, bringing provisions enough to last while the meetings continued. To borrow the language of Dr. Davidson, who has written a history of this revival, "the laborer quitted his task, age snatched his crutch, youth forgot his pastime, the plough was left in the furrow, business of all kinds was suspended, bold hunters and sober matrons, young men, maidens and little children flocked to the common centre of attraction." The usual estimate of the number of people present at Cane Ridge, where the greatest excitement prevailed, is twenty thousand (63:26), though some have thought that this number should be doubled.

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[1] There was a disease in Scotland at about this date, possessing the physical peculiarities of the religious manias, in a somewhat different and exaggerated form. They had fits of dancing, during which they had all the appearance of madness; often they ran with wonderful speed, even over very dangerous ground; sometimes when confined in the house they would climb in a most singular manner, leaping from one cross-beam to another with the agility of a cat, or whirling around one "with a motion resembling the fly of a jack."—*Cf.* Edinburgh Med. and Surg. Jour., Vol. III, p. 434.

A crowd of this kind, oppressed by a "pungent sense of sin," when gathered together for the purpose of worship, would, even with less emotional and impassioned preaching, develop no small degree of excitement,—and this was redoubled under the conditions usually present in these meetings. One must remember that the meetings, which had continued probably the greater part of the day, were prolonged till late at night—indeed, usually even until morning, during all of which time songs were sung, fervent prayers were offered, and perfervid preachers pictured the glories of heaven, or the horrors of hell, in words that burned. The glaring light of camp-fires revealing long ranges of tents through the darkness, the light of flashing torches and flickering candles falling upon the dense mass of people and showing thousands of heads bowed in prayer, and the groans, the sobs and the shrieks of those suffering intense anguish of mind, all conspired to make the scene more weird, "to invest it with terrific interest," and thus produce that delirium of excitement the like of which has never been seen. One other factor contributing to this end was the fact that women and children, as well as men, preached; the preaching of one little girl of seven and a boy of twelve were said to be especially effective (113:499 f.).

The physical effects that followed on these occasions were most remarkable. A great number were seized with an impulse to leap or jump, from which fact the people connected with the revival are generally known in Europe as the "Jumpers," although that was one of the least interesting of the physical characteristics. Some became cataleptic and remained in that condition from a few minutes to several days (113:502). Many were affected with the "jerks," a spasmodic contraction of the muscles which sometimes caused the head to turn from side to side with such rapidity that the features were indistinguishable (113:503); sometimes the whole body was affected and the head was jerked backward and forward so violently that the head almost touched the floor behind and before (113:503), and the reversal of the motion was so sudden tha the hair, if it was long, would "crack and snap like a whip lash" (113:503).[1] According to Barton W. Stone, one of the most prominent religious leaders of his day, it was not alone the weak in body and mind that were subject to these violent convulsions—but "all classes,[2] saints and sinners, the strong as well as the weak" (113:503), and in the same connection he says he has seen some thrown to the earth by the violence of their convulsions while they were cursing the jerks. But while it is true that all classes and all ages, from the child of six or less[3] to the man of sixty were subject to these convulsions (85:5 f.), it is also true that women and children were more commonly affected, and according to Dr. Felix Robertson (p. 5), particularly those from fifteen to twenty-five.

Richard M'Nemar, who was one of the preachers in the revival, and who, as an apologist, would certainly not exaggerate, says (p. 61) that some, stretched at full length, rolled on the ground like a log for hours at a time, others, drawn double, with head and feet together, rolled round and round like a wheel, and still others were dashed to the ground and "bounced from place to place like a football." Evidently it is this last phenomenon which Hecker (p. 148) and McMaster (p. 58) describe in a more fitting figure when they compare the motions of the sufferer to those of a live fish out of water. Others, according to Mooney's report (p. 963), hopped about like frogs.

After the convulsions began in violent form, the sufferer was absolutely powerless,—he was as a leaf in a whirlwind;[4] but when they came in the form of the jerks, which, in the earlier stages, were mere twitchings of the limbs and muscles that move the head, the sufferer might protect himself, in a measure, by holding to some stationary object. In some places, when clearing the ground to make an opening to accommodate the crowds, saplings were cut off breast-high and left "for the people to jerk by." McMaster (p. 581) refers to one place where there was quite a grove of these stumps around which the earth was kicked up "as by a horse stamping flies."

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[1] Dr. Davidson would not record cases of this kind until he got information from eye-witnesses. Dr. Yandell followed his example. I can introduce the same kind of testimony, for my grandmother witnessed these peculiar phenomena among the jerkers in Tennessee.

[2] Lorenzo Dow, one of the revivalists, in describing his meetings at and near Knoxville, Tennessee, says: "I have seen Methodists, Baptists, Quakers, Church of England, and Independents exercised with the jerks—gentlemen and ladies, black and white, rich and poor without exception. Those philosophers who wish to get it to philosophize upon it and the most godly are excepted from the jerks. The wicked are more afraid of it than the small-pox or yellow fever." The Presbyterians might have been added to the list of denominations he gave, for it was among them that the Kentucky Revival began.

[3] Jonathan Edwards (2 : 158) tells of a child of four being so affected in one of his meetings; in fact, Edwards gave special attention to children and was anxious to get as many as possible under this influence (*loc. cit.*).

[4] It was impossible to quiet one who had the convulsions; all efforts in that direction usually made one worse; and besides, one man could scarcely be restrained by five or six, so great was his strength when in this condition (49:144).

Many fell suddenly to the earth as in an epileptic fit; this was called "the falling exercise." One person's falling seemed to be the signal for others; in other words, it was very contagious. Where these vast crowds were assembled, the number of those affected ran into the hundreds; indeed, M'Nemar reports (p. 26) that on one occasion three thousand fell; this statement is made on the basis of an actual attempt to count the "spiritually slain," as those who fell were called. These were taken away, to prevent their being trampled upon by the jumpers, and laid in rows in the meeting-house, which was reserved for this purpose—the preaching being in the open air. Some of these persons were unable to move for quite a while; some could merely kick the floor with their heels; some were soon up and among the jumpers, the jerkers or the rollers, or perhaps rushing wildly about in the forest (62:581).

There was another class known as the "barkers;"[1] according to Brown (p. 343) and M'Nemar (p. 62) both of whom were on the ground, people would get down on all-fours and bark and growl like dogs.[2] Mooney (p. 943) adds that they would get down in front of the preacher, and bark as long as he preached. At first those who had the barks felt very much humiliated at being compelled to do a thing that seemed so degrading, but later they were regarded as possessing a larger measure of the Holy Spirit (69:943); probably it was then that the disease became more persistent, for we are told that some continued their barking "from month to month" (63:62). It scarcely need be mentioned that all who participated in these meetings believed all these phenomena to be the manifestation of the Holy Spirit which had taken possession of the man, soul and body. It is interesting to note that in the Middle Ages, phenomena similar to these were attributed to the devil, and the "possessed" were exorcised by the priests. It is interesting, furthermore, to notice that the faith of those who believed in the divine influence was never shaken by the fact that cold water dashed on a sufferer might cure him.[1]

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[1] Barking manias have broken out from time to time since the fifth century, but they are not necessarily connected with religion. Sometimes the imitation is of a cat, a sheep, or a dove, instead of a dog. See Dict. Psych. Med., Vol. I, p. 436; Havelock Ellis, "Man and Woman" (Ed. 1904), p. 353; Calmeil, De la folie, t. I, p. 513.

[2] This performance has been closely paralleled in recent years by some of the Russian sects. Besides manifesting the more common physical phenomena, including the dance, the whirling of the dervishes, and convulsions, "men and women tear off their garments and go about on all-fours, ride on one another's backs, and give way to the sexual erethism which had been exalted to the highest point" (Ellis, 333-4), yet they call themselves Christs, and actually claim to possess a portion of divinity, and to be worthy of adoration; it is admitted that they are ascetic under normal conditions, but are carried beyond themselves by an excitement which they believe is of divine origin.

The barkers finally learned that dancing would give relief,—that it would as it were, drain off the surplus nervous excitement, and they preferred voluntary dancing to involuntary barking, according to M'Nemar (pp. 62-3). One of the ministers, so the same author says, set the example, and soon the dance came to be regarded as a part of the worship (63 : 60).

The "holy laugh" was another peculiar feature of this revival. Sometimes while the sermon was in progress half the congregation would be laughing aloud in the most serious way—if it be not a contradiction to say so—for they regarded it as a part of the worship (113 : 505).

One would naturally expect many serious injuries if not a few fatalities, from the violent actions of those so powerfully affected, but singularly enough, we find no record of such; on the contrary we are told (18 : 346-7; 69 : 942) that very few hurts were received, and those were not at all dangerous. Evidently those most under the influence of the excitement were largely if not wholly anæsthetic. This was confirmed by accident in one case. A physician administered hartshorn to a young man who had fallen, in order to revive him, and spilled some in his nose, but he took no notice of it (113:502).

There is reason to believe that all the after-effects have not been recorded by the historians of the revival; and this is not strange, for being themselves participants, they might naturally, as apologists, keep silent on the most objectionable features, or at least fail to attribute them to their true cause. In view of the results of such excitement in other places one is surprised not to find record of more cases of permanent insanity. If the New England Revival[2] which did not by any means assume the proportions of the Kentucky Revival, resulted in a mania for suicide (62a : II, 159) it would be expected that mental aberration would not be absent from the latter. It is probable, also, that the health of many was permanently injured; in fact we are told (49:152) that many retained for life some nervous disorder which resulted from the excessive excitement. Interesting cases in a different section of the country will be given in another connection.

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[1] A minister in the Shetland Islands having his sermons frequently interrupted by several members who were subject to these convulsive attacks, announced to his congregation that immersion in cold water was the most effectual remedy known for that trouble, and that in future those who were attacked would be taken to a near-by lake. It need scarcely be said that he never had occasion to use the remedy.

[2] The New England Revival started in 1734 under the preaching of Jonathan Edwards; but the extravagances of this were far surpassed by the Great Awakening which occurred five years later. This was

While the Kentucky Revival proper ended about 1805 (having begun, as usually reckoned, in 1800), that variety of religious excitement has continued even down to the present. A wave which would have been considered remarkable but for being overshadowed by that which we have described, overran Ohio, New York and some of the New England states about the middle of the last century; but since that time it has constantly decreased as an epidemic, and grown milder in form. I have studied the question at first-hand in some of the Southern states which were so much affected by the Kentucky Revival, and I find that jumping, or shouting as it is now usually called, is practically the only "bodily affection" in recent years, and even this is not common. Other sections of the country now have severer forms than I have met with even among the negroes of the South within the last decade. The Holy Rollers were to be seen in Worcester, Mass., only two or three years ago; the Holy Jumpers are found in several of the Western states to-day. The Shakers in Maine, Prince Edward Island, and perhaps in other places, still present the same peculiarities, and consider dancing a part of the divine service.

Since the great revival, and particularly in the last quarter of a century, a number of sects have sprung up which make even more extravagant claims than those a century ago. Like these, they claim to see visions and to speak with tongues; many of them claim to work miracles, and some claim to be Christ reincarnate. The Millerites, founded in 1831, and now numbering about twenty thousand, still hold together, notwithstanding their disappointment at Christ's failure to return to earth in 1843, as they expected. The Beekmanites, founded in Illinois in 1875, by Mrs. Beekman, and now presided over by her successor, Schweinfurth, both of whom have claimed to be Christ, still gain new members. In 1888, a man by the

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the time of the excitement aroused in England by the Wesleys and Whitefield; in fact, they made a visit to America during this revival, and Whitefield took quite an active part in it. All such convulsive religious epidemics are regarded by Wicke, Hecker and Haeser (44 : II, 179) as forms of the dancing mania. Fort (p. 363, 5) agrees with these writers as to the outward resemblance of the religious to the dancing epidemic, but he differs as to the cause.

name of Brown, near Chattanooga, Tennessee, claimed to be the risen Christ, and was actually worshipped as such. Some of his followers offering to die to prove their faith, the sheriff stepped in and drove Brown from the neighborhood. A year later, a similar craze arose among the negroes of Georgia and South Carolina. A number of negroes claimed to be Christ, and a sect known as the "Wilderness Worshippers" was formed, and a temple and an ark were built. When their enthusiasm led to the killing of a woman, the leaders were adjudged insane and sent to the asylum; many others were put under arrest and held in custody till the excitement died out. At the same time, Indiana, Illinois and Missouri were visited by the "Heavenly Recruits," who showed the same extravagance of mental and physical phenomena, till the civil authorities "put a stop to the mental and physical demoralization (69: 944 ff.). "Zion" and "Shiloh" being just now at their height, need but to be mentioned. The Doukhobors, one of the most interesting of modern sects, persecuted by the Russian government, defended by Tolstoy, and, finally, allowed to seek refuge in Canada, must also be omitted here.

Such striking parallels are found among the Indians that it will be well to mention one or two of them before leaving this subject. The Indians of Puget Sound have a sect of Shakers; it was founded in 1882 by an Indian, John Slocum, who mistook his delirious dreams in a spell of sickness for a heavenly vision; who, in short, believed he had been to the gates of heaven, and had talked with Christ face to face. Having, as they believe, a direct revelation, they do not care for the Bible. While their worship is characterized by some extravagances, especially by dancing, their life is above reproach. They make special war on drunkenness, gambling and such vices, they preach honesty, sobriety, temperance and right living, and they practice what they preach (69: 759).

The Ghost Dancers originated about 1890, through the work of Wovoka, another Indian who saw "visions," and was transported to heaven during delirium, caused by fever. Though generally known as the "Messiah," and usually believed to have assumed that title himself, he repudiates all claims to divinity, but he does claim to be in constant communication with God, and maintains that he has been given power over the elements; he actually opened negotiations with the powers at Washington "to keep the people of Nevada informed of all the latest news from heaven, and to furnish rain whenever wanted, for a small consideration paid at regular intervals" (69: 773).

Their ceremony consists in forming a circle, joining hands, and marching around the leader, singing songs of suitable rhythm; this is kept up till some become entranced, or as they express it, "go to the spirit world." Most who go into trance become cataleptic, and are usually allowed to fall heavily to the ground. They may remain in this rigid and unconscious condition for a few minutes only, or for hours. In many instances, the entranced person will spin around like a whirling dervish, or maintain some difficult posture for a length of time that would be impossible for one in the normal condition (69 : 925 ff.). Mr. Mooney, of the department of Ethnology at Washington, saw a young Arapaho become rigid in trance night after night. According to him (p. 924), this young man took part in the terrible sun dance later, "dancing three days and nights without food, drink or sleep."

The belief that the Messiah was going to restore the old order of things and make all Indians invulnerable caused the dance which he instituted to be generally engaged in, and the excitement to which it led to become more wide-spread. It was this excitement added to hunger that brought to a focus the disturbance that led to the tragic death of Sitting Bull (69:845 ff.). This failure of their prophecies to come true shook their faith and caused the Ghost Dance almost to cease for a time in certain tribes.

There is a peculiar religious disease variously known (according to its form and also to the place where it is found) as lata, lattah, ikota and klikuschi. In America and South Africa it is found without the religious aspect. In this country the people who are affected with it are called the "Maine Jumpers" or "Jumping Frenchmen." According to Dr. Beard, who made a personal study of them (10:170-92), they are, as a rule, mere automata, obeying every command and imitating every action, regardless of danger to themselves or others.[1] Consequently they suffer much at the hands of thoughtless or cruel persons. Some of them who are hotel waiters have, when commanded, dropped dishes, or thrown glasses across the room; others have been made to strike their fists through glass doors, and even jump off high embankments. Similar tricks are played on the Javanese, who suffer from lata. According to Mr. Havelock Ellis (p. 336) if one throws off his coat in the presence of a woman who is affected with this disease, she will immediately undress or do any other indecent act that she sees any one else do, yet, at the same time, she may be very indignant, and may all the while abuse the one who causes her to do such things. According to this same writer, a man who was subject to lata was playing with his child, when a bystander taking up a piece of wood began throwing it up and catching it; the father did the same. When the bystander opened his arms and let the wood fall to the floor, the father, through irresistible imitation, opened his arms, and his child fell to the floor and was killed.

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[1] For much personal information concerning the Jumping Frenchmen, I am indebted to Mr. H. L. Brittain who lived among them in Canada.

It may be well in closing to summarize briefly what has been said in regard to the relation of the general mental condition, or tone of thought, to many of the epidemics considered, and to add to this some further illustrations.

For centuries before the period with which we are primarily concerned, the minds of all had been steeped in superstition. "Vulgar traditions and unreliable legends," says Fort (p. 362), "repeated nightly around the blazing fires of each domestic hearth, or gravely narrated to awe-struck multitudes at fairs, public markets and on great church festival days, assisted in maintaining an unreasonable spiritual activity; the mediæval mind was held to an abnormal strain by the impossible narration written in monastic annals." The imagination was kept at fever heat; miracles were supposed to be taking place every day; demonism was never questioned, and the ever increasing belief in witchcraft prepared the way for various other delusions. The breakings out in convents, of such frequent occurrence in the Middle Ages, were due to the mental food, to uninterrupted religious thoughts and practices, which, combined with the grossest superstitions, and accentuated by a life of solitude at times rendered the inmates of convents quite abnormal. It is safe to say that exaltation of spirit which came as a result of constant worship and prayer, combined with the constant fear of persecution, prepared the way for the epidemic martyrdom of the early Christian church.

Fort, speaking of the dancing mania, says (p. 363): "Ardent fanaticism developed from abnormal religious frenzy and irrational social life and aggravated mediæval superstitions, conjoined to extreme dread of swiftly destructive maladies, prepared the popular mind for the singular disease which swept through Germany in 1374." By "swiftly destructive maladies" he probably meant the plague, primarily, for it had already returned several times since 1348. He may have referred, also, to St. Anthony's fire, that terrible disease which burned away the flesh, and let the joints drop apart, so that a limbless trunk might be found by the roadside praying for death or begging to be killed (24:I, 54). This disease, it is believed, turned the minds of many toward the Holy Land, *i. e.*, inspired them with a desire to join in the crusades. It is possible that the English Sweat, another terrible disease, which occurred first in 1485, and returned four times by 1551, had considerable influence on the later epidemics.

When we come down to the beginning of the modern period, where we find remarkable examples of religious epidemics, especially in England, we find suitable mental food. There were political upheavals, confiscations and consequent ruin, want and brutal treatment; hatreds were intense, persecutions, cruel and bitter, and as a result, men's minds gave way under the strain. Many lived on the borderland between sanity and insanity; and were in a proper mood to be influenced by reported prophecies and miracles which were so common at that time. Even the great revivals of the last century were, as a rule, preceded by prophecies of Christ's second coming, or of the end of the world—reports that, when believed, move men as nothing else can. What the Rev. Mr. Eells says of the Shakers of Puget Sound, the Indian sect founded by a man who believed he had been to heaven, would apply equally well to the religious epidemics previously considered: "When superstitions, ignorance, dreams, imagination, and religion are all mingled together they make a strange compound," and, he might have added, they produce strange results.

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NOTE. The following works have been consulted in the foregoing study.

The references, because of their vast number, have not been given in full in the text nor in foot notes, except where there was a special reason for it. I have adopted and slightly modified the system of references used by Dr. Alexander F. Chamberlain in his book, "The Child." In most cases the reference is by the bibliography number rather than by the name of the author; however, where the author's name is a sufficient index, the bibliography number is omitted, and only the page is given, or the page and volume in case of works of more than one volume. For example, "Sprengel (II, 25)" would refer to Vol. II, page 25, of the work given in the bibliography under "Sprengel;" in a reference of this form (50:75) the 50 would locate the work in the bibliography, and 75 would indicate the page; if the work has more than one volume, the volume is referred to thus (44:II, 50).

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MATHEMATICAL PRODIGIES.[1]

By FRANK D. MITCHELL.

The object of the present paper is threefold:

(1) To give a summary of the mathematical prodigies[2] described in the literature of the subject, without, however, duplicating unnecessarily the work of previous writers.

(2) To give a brief account of the writer's own case, which is, it is believed, fairly typical, despite certain peculiar limitations to be described later, and which will shed light on certain factors in mental calculation that have not hitherto received full recognition.

(3) To set forth a new theory of mental calculation, based upon the foregoing data, and incidentally to criticise certain other theories hitherto advanced in this field.

I.

In view of the incompleteness of existing data in most cases, and the inaccessibility of some even of the existing sources of information, a complete history of the mathematical prodigies would be out of the question. We shall, therefore, simply attempt to give a reasonably complete list of those of whom definite information is available, together with a statement of the significant facts known about them. A few names—that of Euler, for example—have been omitted on account of the absence of any satisfactory data that would shed light on the theory of mental calculation; and no attempt has been made to collect the accounts of new prodigies found every now and then in the newspapers. Such accounts are not readily accessible, and are usually so popular and unreliable that they have little scientific value.

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[1] From the Psychological Seminary of Cornell University.

[2] By a "mathematical prodigy" we shall mean a person who shows unusual ability in mental arithmetic or mental algebra, especially when this ability develops at an early age, and without external aids or special tuition. We shall use the word "calculator" in the sense of "mental calculator," as a synonym for "mathematical prodigy," and shall usually mean by "calculation" "mental calculation," unless the contrary is clearly indicated by the context. A "professional calculator" will be taken to mean a mental calculator who gives public exhibitions of his talent. "Computer," however, will be restricted to mean one who calculates on paper. All problems mentioned as solved by the mathematical prodigies will be understood to be done mentally, unless otherwise indicated.

There are several possible bases for a classification of the mathematical prodigies. We might group them chronologically, as Scripture[1] does; or by the extent of their power, as measured either by the size of the numbers they could handle or by the rapidity of their calculations; or by the degree of their mathematical ability, as shown by the character of the problems they solved and the processes they used. Or we might classify them according to memory type, as either visual or auditory calculators. No one of these classifications, adhered to consistently throughout, would quite answer the purpose here, owing to the great unevenness of the material at hand in the case of the different calculators. An arrangement has therefore been adopted which is in part chronological, but which is modified by most of these other considerations. In this way, so far as the crossing of the different principles of division permits, those men are in the main brought together who are most naturally compared, and the important points of resemblance and difference come out more conveniently than if an abstractly logical arrangement were adopted.

We begin, then, with Fuller and Buxton, who have much in common, and who are the first modern calculators about whom reliable data are available. Colburn, Mondeux, and Inaudi form the next group, followed by Zaneboni, Diamandi, and Dase. Then come the two Bidders and Safford, followed by Gauss and Ampère, and finally those who may be called "minor prodigies," whether because of limited powers of calculation or because the available information is not sufficient for a more detailed account.

Tom Fuller[2] (1710-1790), "the Virginia calculator," came from Africa as a slave when about 14 years old. We first hear of him as a calculator at the age of 70 or thereabouts, when, among other problems, he reduced a year and a half to seconds in about two minutes, and 70 years, 17 days, 12 hours to seconds in about a minute and a half, correcting the result of his examiner, who had failed to take account of the leap-years.[3] He also found the sum of a simple geometrical progression, and multiplied mentally two numbers of 9 figures each. He was entirely illiterate.

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[1] In his article on "Arithmetical Prodigies," in the *American Journal of Psychology*, IV, 1891, pp. 1-59. We shall hereafter have frequent occasion to refer to this article, the only one in English in which a comprehensive study of the subject is attempted.

[2] Scripture, *op. cit.*, p. 2; Binet, *Psychologie des grands calculateurs et joueurs d'échecs*, 1894, p. 4; *American Museum*, V, 1789, p. 62. This last date is erroneously given by Scripture as 1799.

[3] Binet, *op. cit.*, p. 5, notes that the harder problem was done in less time than the simpler one, and is inclined to suspect that the records are unreliable. But in the case of so slow and plodding a calculator

Jedediah Buxton[1] (1702-1772) was very stupid even from boyhood. Though his father and grandfather were men of some education, he remained illiterate all his life, and was of less than average intelligence; even the statement of a mathematical problem he comprehended, we are told, "not without difficulty and time." In calculation he was, like Fuller, extremely slow; but he had a prodigious memory, and could retain long numbers for days or even months, so that he performed enormous calculations, which in some cases occupied him for weeks. On one occasion he mentally squared a number of 39 figures, in 2½ months. His methods were original, but very clumsy; to multiply by 378, in one instance, he multiplied successively by 5, 20, and 3 to get 300 times the number, then by 5 and 15 to get a second partial product, and finally by 3, to complete the operation. Thus instead of adding two zeros to multiply by 100, he multiplied first by 5 and then by 20. This fact, together with his slowness, shows pretty clearly that his methods were of counting rather than multiplication, though we are told that he had learned the multiplication table in his youth. He could give from memory an itemized account of all the free beer he had had from the age of 12 on. He was able to calculate while working or talking, and could handle two problems at once without confusion. At a sermon or play Buxton seems to have paid no attention to the speaker's meaning, but to have amused himself by counting the words spoken, or the steps taken in a dance, or by some long self-imposed calculation. He could call off a number from left to right or from right to left with equal facility, and by pacing a piece of ground could estimate its area with considerable accuracy.

Zerah Colburn[2] (1804-1840), the son of a Vermont farmer,

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as Fuller, little importance can be attached to such discrepancies, especially since the times given are only approximate. Moreover, Fuller was at this time about 70 years old himself, and may therefore have had in his memory, already calculated, the number of seconds in 70 years. The times given seem to indicate that he used a process of modified counting, rather than multiplication in the ordinary sense. The importance of this distinction will appear later.

[1] Scripture, *op. cit.*, p. 3; *Gentleman's Magazine*, XXI, 1751, pp. 61, 347; XXIII, 1753, p. 557; XXIV, 1754, p. 251.

[2] Also spelt *Colborne*. Scripture, *op. cit.*, p. 11; *A Memoir of Zerah Colburn, written by himself*, Springfield, 1833; *Philosophical Magazine*, XL, 1812, p. 119; XLII, 1813, p. 481; *Analectic Magazine*, I, 1813, p. 124; Carpenter, *Mental Physiology*, §205, p. 232; *Cornhill Magazine*, XXXII, 1875, p. 157; *Belgravia*, XXXVIII, 1879, p. 450; Gall, *Organology*, §XVIII, pp. 84-7 (in *On the Functions of the Brain*, V, Eng. tr., Boston, 1835). Scripture gives two other references which

was regarded as a backward child until the end of his 6th year, when one day his father heard him repeating parts of the multiplication table, though the boy had had only about six weeks' schooling. The father then "asked the product of 13 x 97 to which 1261 was instantly given in answer. He now concluded that something unusual had actually taken place; indeed he often said he should not have been more surprised, if some one had risen up out of the earth and stood erect before him."[1] The elder Colburn now took Zerah about the country, giving public exhibitions of the child's powers in various cities. Colburn was thus the first professional calculator, in the sense already defined. From the list of questions answered by him at Boston, in the fall of 1810, and from the account in the body of the *Memoir*, it appears that even at this early date, only four months after the discovery of his talent, he was a good calculator, though of course he improved with further practice. It is clear, therefore, that his powers had been developing for some time—to judge from other cases at least six months, if not a year—before they attracted his father's attention. This may mean that he learned to count from his elder brothers and sisters,—the eldest was about seven years older than Zerah,—rather than from his own brief six weeks at school. Colburn's preference for multiplication, the extraction of roots, factoring, and the detection of primes seems to have developed early; he never became as proficient in division as Bidder, for example, and, like most of the prodigies, he used addition and

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the writer has been unable to consult: *The Amerian Almanac*, 1840, p. 307, and the *Medical and Philosophical Journal and Review*, III, 1811, p. 21. Gall's account, however, seems to be based upon this last article.

[1] *Memoir*, pp. 11-12. Scripture (*op. cit.*, p. 12) is "tempted to ask for the authority on which the statements were made", and inclined not to "put too much faith in the figures", on the ground that Colburn never speaks of himself as having any extraordinary power of memory for long periods of time. But the full passage as quoted above makes it clear that the father had told the incident repeatedly to awe-stricken listeners in Zerah's hearing; moreover, the remembering of such a simple problem could hardly require "extraordinary power of memory" in a person used to mental calculation. Colburn's feats in factoring large numbers are hard to explain except by supposing that he remembered at least those numbers which he had previously examined and found prime. This would imply a rather considerable development of his memory for figures. At any rate, there is nothing improbable in his remembering the figures quoted in the text, even for some years after his calculating powers had declined.

It may be noted that later in his article Scripture's faith in Colburn's memory increases; for on page 46 he thinks we can presuppose in the case of Colburn and certain others an extended multiplication table, perhaps even to 100 x 100. Reasons for rejecting this supposition, in Colburn's case at any rate, will appear later.

subtraction only incidentally, in the service of other operations, not for their own sake. In answering catch questions and in repartee he was moderately clever.

In the spring of 1812 Zerah was taken by his father to London. Here, among other feats, he found mentally, by successive multiplication, the 16th power of 8 (= 281,474, 976,710,656) and the 10th powers of other 1-figure numbers, also, though with more difficulty, the 6th, 7th, and 8th powers of several 2-figure numbers. The square root of 106,929 (= 327) and the cube root of 268,336,125 (= 645) were found "before the original numbers could be written down." He immediately identified 36,083 as a prime number, and found "by the mere operation of his mind" the factors, 641 and 6,700,417, of 4,294,967,297 ($=2^{32}+1$).[1]

While in London, Colburn learned to read and write, and later began the study of Algebra; but his education was subject to long interruptions, owing to the constant financial difficulties caused by his father's lack of business ability. After visits to Ireland and Scotland, the Colburns went, in 1814, to Paris, where Zerah spent eight months at school, studying mainly

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[1] *Memoir*, pp. 37-8, quoting from a prospectus printed in London, 1813. From Colburn's own account of his methods of factoring (pp. 183-4), it appears that the only way in which he could *immediately* identify as prime such a number as 36,083 would be by remembering the result of a previous examination of it. Scripture (*op. cit.*, p. 14, note) says that it "requires considerable faith" to accept the statement that Colburn factored $2^{32}+1$. But we are *not* told that he did it "instantly"; a friend of Morse's says simply, "almost as soon as it was put to him" (Scripture, *loc. cit.*, quoting from a letter in S. I. Prime's *Life of Samuel F. B. Morse*, p. 68; the reference is undoubtedly to this problem), while Carpenter (*Mental Physiology*, p. 233; the writer has not been able to find Carpenter's authority for this statement) says, "*after the lapse of some weeks*." Even if the time was only a matter of some minutes, the feat is not incomprehensible. The smaller factor, 641, might easily have been hit upon by a lucky trial at a very early stage of the work. We read in Baily's account (*Analectic Magazine*, I, 1813, p. 124) that "any number, consisting of 6 or 7 places of figures, being proposed, he [Colburn] will determine, with . . . expedition and ease, all the factors of which it is composed." Now $2^{32}+1$ is only a 10-figure number, or three figures longer than those Colburn was used to handling; and the smallness of the factor 641 renders the problem much simpler than it at first appears. Since, then, the feat is entirely possible, and since it is cited by Colburn from the publicly circulated Prospectus of 1813, and is mentioned by at least one contemporary writer who was not acquainted with the *Memoir*, there is no reason for believing that Colburn fabricated the incident; especially since his limited mathematical knowledge would never have shown him the importance of this particular number. Had he been inventing out of whole cloth, he would have multiplied together two prime numbers chosen at random, and would probably have made the smaller one at least a 4-figure, if not a 5-figure number. On the historical reliability of the *Memoir* see Appendix I.

French and Latin. Returning to England early in 1816, he entered Westminster School in September, under the patronage of the Earl of Bristol, making fair progress in the languages, and standing well in his class, in which, however, he was one of the oldest boys. He also studied six books of Euclid under a private tutor, but showed no marked geometrical aptitude. In 1819 his father removed him from school, and soon after we find him, at his father's suggestion, unsuccessfully attempting the career of an actor and playwright. In 1822 he opened a small school, which ran for a year or more. His next occupation was as a computer in the service of the secretary of the Board of Longitude. Shortly after his father's death, in 1824, Zerah returned to America, and in December of 1825 joined the Methodist church, becoming a circuit preacher. After seven years of this occupation,[1] being in need of funds to eke out his modest ministerial salary, he wrote the *Memoir*, carrying out a plan which his father and friends had had in view long before. In 1835 he resumed teaching, as "Professor of the Latin, Greek, French and Spanish Languages, and English Classical Literature in the seminary styled the Norwich University."[2] He died in 1840.

From this brief account of Colburn's romantic career, it will be seen that his education, while much interrupted, was fairly good. He spent four or five years in the study of languages, for which he seems to have had a natural liking, and later was able to teach them. He began the study of algebra, but did not get beyond the elements of it; and he studied geometry, which he found easy but uninteresting, owing to the lack of any visible practical application. The literary style of his *Memoir*, though far from Addisonian, is always readable, the book is interesting throughout, and even the specimens of his poetry given in the appendix are not specially bad, all things considered. The question of the historical reliability of the *Memoir* will be discussed later; for the present it will suffice to say that, on a careful reading, the book shows scarcely a trace of that self-glorification with which it has been charged by Scripture and Binet.

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[1] *I. e.*, in 1832 or 1833. *Cf. Memoir*, p. 31, "after possessing the talent twenty-two years", from August, 1810; p. 142, "nine years' residence here" in America, from June, 1824; p. 166, "twenty-two years ago", to 1810 or 1811; p. 167, "the last seven years that he has spent in the traveling connection", from December, 1825. These passages show that the *Memoir* was not begun, or at any rate had not reached the third chapter, before 1832, and was not completed until shortly before its publication in 1833. Scripture's statement, therefore (*op. cit.*, p. 11, note [2]), that "there is no statement regarding the time at which they [the *Memoir*(s)] were written, or even a date to the preface; the last year mentioned in the book is 1827", is decidedly misleading. The last date printed in figures, to be sure, so that it could be identified by a cursory glance, is 1827; but the last date "mentioned" is certainly 1832, if not 1833, even granting that all the periods of time above quoted are only approximate, and cannot be taken without an allowance of half a year one way or the other for possible error.

[2] Scripture, *op. cit.*, p. 16, quoting from *American Almanac*, 1840, p. 307.

Concerning the rapidity of Colburn's calculations not much is known. The only series of problems whose times he gives us dates from 1811, before he was 7 years old, and so is hardly typical of his performances two or three years later when he was in his prime. The times indicated are fairly short, in most cases shorter than if the work had been done on paper by a good computer. The testimony of observers as to his "extraordinary rapidity" is of little value in the absence of definite figures; especially since some of his feats, notably the extraction of square and cube roots and the finding of factors, were accomplished by the aid of extremely simple methods. Colburn's powers probably increased up to the time of his visit to Paris in 1814; but when he gave up his regular exhibitions, and became interested in other matters, he gradually lost much of his skill. There seems to be no authority, however, for the statement[1] that after a time his powers left him entirely; in 1823, at any rate, after a considerable period of disuse, they were readily revived for purposes of written longitude computations.

Of his methods of calculation Colburn has left us a very good account; the only calculator of whom we have a fuller account is Bidder,[2] whose methods closely resembled Colburn's. Both men, in multiplication, began at the left, instead of at the right as we usually do in written computations; and both, by the aid of certain properties of the 2-figure endings[3] of the numbers used, were able to find with remarkable ease and rapidity the square and cube roots of exact squares and cubes, and also, though less rapidly, the factors of fairly large numbers.

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[1] Scripture, *op. cit.*, p. 15.

[2] Bidder's account is more detailed, better written, and in more concise mathematical language than Colburn's, as a result of Bidder's superior educational advantages; it contains, furthermore, explanations of several of Bidders's feats, such as the solving of compound interest problems, which would have been hopelessly beyond Colburn's powers. At the same time Colburn's account is perfectly clear, to the non-mathematical reader perhaps even clearer than Bidder's. In this matter, as in several others, Scripture is hardly fair to Colburn; thus he speaks of Colburn's explanations as "the least intelligible of all the explanations" (p. 50). It is no reproach to Colburn that he was excelled by Bidder; but he certainly deserves credit for what he did do, and one of the things he did was to write a very good account of his methods, over twenty years before Bidder followed his example.

[3] By a 2-figure ending we shall mean the last two figures of any given number; thus 56 is the 2-figure ending of 3456, 01 of 2401, 07 of 7, etc. What properties of these endings were used by the mental calculators will be explained hereafter.

Colburn had two physical peculiarities that need to be mentioned. (1) He possessed an extra finger on each hand and an extra toe on each foot. This peculiarity he shared with his father and two[1] of his brothers. (2) In his early years his calculations were accompanied by certain bodily contortions, similar to those of St. Vitus' dance. They seem to have passed away rather early; Colburn himself has no recollection of them, and mentions them simply on the authority of persons who saw him when "quite a child."[2]

Henri Mondeux[3] (1826-1862) was the son of a woodcutter near Tours. Sent to tend sheep at the age of 7, he amused himself by playing with pebbles, and thus learned mental arithmetic. Jacoby, a schoolmaster at Tours, hearing of him, sought him out, offered to instruct him, and gave him his address in the city; but the boy's memory outside mathematics was so poor that he forgot both name and address, and found the schoolmaster only after a month's search. He received instruction in arithmetic and other subjects, and in 1840 was exhibited before the Paris Academie des Sciences. In the committee's report on him we are told that he "carries on readily in his head not only the various arithmetical operations, but also, in many cases, the numerical solution of equations; he devises processes, sometimes remarkable, for solving a great number of different questions which are ordinarily treated by algebra, and determines in his own way the exact or approximate value of integral or fractional numbers which satisfy given conditions." More specifically, he finds powers of numbers by rules of his own discovery which are equivalent to special cases of the binomial theorem; he has worked out formulas for the summation of the squares, cubes, etc., of the natural numbers, and for arithmetical progression and other series; he solves simultaneous linear equations by a method of his own, and sometimes equations of higher degree, especially where the root is a positive integer; and he solves such problems in indeterminate analysis as finding two squares whose difference is a given number. He "knows almost by heart the squares of all whole numbers under 100." Learning a number of 24 figures, divided into four 6-figure periods, requires 5 minutes. He can solve a problem while attending to other things.

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[1] Colburn says (*Memoir*, p. 72), "his father and two of his [father's] sons," while the account in the *Philosophical Magazine* (XLII, 1813, pp. 481-2) says Zerah and three of his brothers. It has been assumed in the text that Zerah did not count himself, and that the other writer counted him twice; this is the simplest way of reconciling the two statements. The peculiarity had been in the Colburn family, we are told, for several generations.

[2] *Memoir*, p. 173. Scripture does not refer to this second peculiarity; but since Colburn mentions another mathematical prodigy with a similar affliction, and since Safford showed a striking nervousness in his early calculations, it has seemed worth while to mention the matter. Gall, probably quoting from the *Medical and Philosophical Journal and Review* article already cited, seems to refer to this nervousness when he says (*op. cit.*, V, p. 86): "While he [Colburn] answers, it is seen, by his appearance, the state of his eyes, and the contraction of his features, how much his mind labors." Colburn was not quite 7 years old when seen by the writer of the article on which Gall's account is based. Gall himself, however, examined Colburn in Paris, probably in 1814. *Cf. Memoir*, pp. 76-7.

[3] Scripture, *op. cit.*, p. 21; *La grande Encyclopédie*, art. *Mondeux;* Cauchy's report on Mondeux, in *Comptes rendus hebdomadaires des séances de l'Academie des Sciences*, XI, 1840, pp. 840, 952; reprinted in *Oeuvres Complètes de Cauchy*, 1e Série, 1885, V, p. 493, and in Binet, *op. cit.*, pp. 14-22. The writer has been unable to consult the other references cited by Scripture.

Mondeux's admirers hoped that he would one day distinguish himself in a scientific career; but this was not the case. Like his successor Inaudi, whom he closely resembles in several respects, he became a professional calculator; but he had no ability outside of mathematics, and even there his powers soon reached a limit beyond which they did not increase. He died in obscurity. If we may judge by the Academy report, he was almost the equal of Bidder in his insight into mathematical relations;[1] but on the numerical side he was far excelled by Inaudi, who could, for example, memorize 24 figures in half a minute, a feat for which Mondeux required 5 minutes.

Jacques Inaudi[2] (b. 1867), an Italian by birth, passed his early years, like Mondeux, in tending sheep. An anecdote which Binet regards as rather doubtful indicates a possible prenatal influence in the direction of calculation; otherwise there is nothing noteworthy in his heredity. His passion for figures began about the age of 6, and at 7 he could carry on mentally multiplications of 5 figures by 5 figures. His education is very slight; he did not learn to read and write until he was 20 years old. Outside of mental calculation he has no special ability; his memory for most things except figures is rather poor, and he is often absent-minded. At last accounts he was still a professional calculator, living by public exhibitions of his talent. He visited the United States in 1901-2, appearing in many of the larger cities, and is said to have been fairly well received by American audiences.

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[1] Just how much Mondeux owed to Jacoby's teaching is hard to say. The writer has been unable to consult Jacoby's *Biographie d'Henri Mondeux* or Barbier's *Vie d'Henri Mondeux;* Binet, however, who cites both these works, says that Jacoby's lessons were "sans grand succès." (*Op. cit.*, p. 14.)

[2] Binet, *op. cit.*, pp. 24-109, 199-204, *et passim.*

Telling on what day of the week a given date falls is one of his favorite problems. The reduction of years, months, etc., to seconds he accomplishes almost instantly, knowing by heart the number of seconds in a year, month, week, or day. He solves by arithmetic problems corresponding to algebraic equations of the first and sometimes of higher degree, also such problems as the resolution of a given 4- or 5-figure number into the sum of four squares. In these latter cases, however, he proceeds for the most part simply by trial, aided, of course, by his skill in calculation and his familiarity with many squares, cubes, and the like. At his regular performances the programme includes the subtraction of one 21-figure number from another, the addition of five 6-figure numbers, the squaring of a 4-figure number, the division of one 4-figure number by another, the extraction of the cube root of a 9-figure number and the 5th root of a 12-figure number, or such similar problems as may be proposed by the audience. As each number is announced he repeats it slowly to his assistant, who writes it on the blackboard and then reads it aloud, to make sure there is no mistake. Inaudi then repeats the number once more, after which he devotes himself to the solution of the problem, meanwhile making an occasional remark to keep the audience in good humor. Throughout the exhibition he faces the audience, never once looking at the blackboard. Actually he begins his calculation as soon as the numbers are given, and carries it on during the various repetitions of the numbers by himself and his assistant, so that by the time he seems to begin the solution he may be well advanced toward the answer. In this way he appears to work much more rapidly than he really does.

Inaudi is a well-marked instance of the auditory[1] memory type. When he thinks of numbers, in calculation or otherwise, he does not see them "in his mind's eye," as arrays of dots or other small objects, or as written or printed figures; numbers are for him primarily *words*, which he hears as if spoken by his own voice, and during his calculations he almost always pronounces at least some of these words, either with partial distinctness or in a confused murmur. Any interference with this habitual articulation embarrasses him, and prolongs his calculation. He remembers a number very much more readily after hearing it than after seeing it; in fact, if a written number is handed to him, he usually reads it aloud, in order to learn it by sound rather than by sight. Whether visual images are *entirely* absent is a purely theoretical question; it is at least clear that, if present at all, they play a negligible part in his mental computations. We shall later find reason to believe that this condition is by no means so rare as has been supposed. Owing to the traditions of English and French psychology, the visual theory of mental calculation has lain ready to hand, and has in the past found much apparent confirmation. But now that an unmistakably non-visual calculator is on record, it will no longer do to beg the whole question; we must insist on considering each case upon its own merits, either settling it by definite evidence or leaving it frankly in doubt. We shall see later how much of the supposed evidence for the visual theory falls before a careful examination.

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[1] Actually it would be more correct to call his type auditory-motor, and the same is probably true of most of the other auditory calculators we shall study, since a pure or non-motor auditory individual is rare. For convenience, however, the writer has followed Binet's terminology. The meagreness of our information in most cases makes it difficult to tell just what part the motor element plays; and this is especially true when we are dealing with a limited field like calculation, where the motor element may often play a less important part than in certain other fields.

One of Inaudi's most marked characteristics is his powerful memory for figures. In one experiment he was able to repeat, after a single hearing, though with an effort, 36 figures, read off to him slowly in groups of three; but in the attempt to repeat 50 figures under the same conditions he became confused, and got only 42 of them correct. This latter number, 42, Binet therefore takes as the limit of Inaudi's power of acquisition, or "mental span," under these conditions. In an experiment made to determine in what time he could learn 100 figures read off to him in groups as often as requested, he learned the first 36 in a minute and a half, the first 57 in 4 minutes, 75 in 5½ minutes, and the whole 100 (actually there were 105) in 12 minutes. On the other hand, he can repeat in order, at any time within a day or two, all the figures used in his last performance, whether in the statement of the problems, in the answers, or in the intermediate calculations. The number of these figures at times runs as high as 300, and the total duration of the performance is usually not more than 10 or 12 minutes. Each new performance, however, blots out of his memory almost entirely the figures used in the previous one; but such constants as the number of seconds in a year, etc., as well as many powers and products, and any particular numbers or results in which he for any reason takes a special interest, remain permanently with him. These facts show how important it is to take account of the conditions of such experiments if the figures established by them are to have scientific value. In an experiment lasting the same length of time as one of his regular exhibitions, but under very different conditions, Inaudi can learn only a third the number of figures he remembers with ease under his usual conditions. In these public performances, however, each number in the problem as given is repeated several times (twice by Inaudi himself, and once each by his assistant and the proposer of the question), and the figures of the various calculations and the result have a logical connection in the problem. Moreover, the numbers are learned in relatively short stages, separated by intervals in which they can be assimilated.[1]

Concerning the rapidity of Inaudi's calculations we have fairly full information,—so much fuller, in fact, than we have for any previous calculator, that no satisfactory comparisons can be made. Since the results of Binet's experiments are readily accessible, a brief summary of them will here suffice. In each experiment the subject was given a written column of numbers, each of which was to be mentally increased or diminished, multiplied or divided, by the same number; in other words, the addend, subtrahend, multiplier, or divisor was uniform for the whole given column of numbers. The results were called off down the column as fast as obtained, and the average time for each single operation thus determined. These tests were made on some of Binet's pupils, on Inaudi, and on four department store cashiers who were thoroughly practiced in addition, subtraction, and multiplication of small numbers, and could perform mentally 2-figure multiplications,[2] and in some cases, though with difficulty, 3-figure multiplications. The students were of course considerably slower than Inaudi and the cashiers; but the cashiers, in dealing with the smaller numbers to which they were accustomed, were fully as rapid as Inaudi, in some cases slightly more rapid. In dealing with larger numbers, however, which exceeded the limits of their customary calculations, their inferiority to Inaudi was very marked.

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[1] Mondeux, it will be remembered, required 5 minutes to learn 24 figures, whereas learning this number of figures is a common incident of Inaudi's exhibitions, and takes only half a minute. Here again, however, the results are not directly comparable. Mondeux learned the number in groups of 6 figures, and presumably from a paper or blackboard, while Inaudi always groups numbers in periods of three, and learns them by audition instead of vision. We shall refer later to a distinction which must be made between the direct and immediate remembering of figures which results from deliberately committing them to memory, and the very rapid and abbreviated automatic calculations which in some of the prodigies simulate direct memory. Recollection as the result of repeated calculation may form an intermediate stage in the passage of the latter into the former. These distinctions will become important in connection with the much discussed question whether, and to what extent, the mental calculators possessed extended multiplication tables.

[2] By a 2-figure, or n-figure, multiplication will be understood hereafter a multiplication in which each of the two numbers contains 2 (or n) figures, and the product 3 or 4 ($2n-1$ or $2n$) figures.

Ugo Zaneboni[1] (b. 1867), an Italian, born in the same year as his countryman Inaudi, received a fair education. His interest in numbers began at the age of 12, and when 14 he could solve[2] any problem his teacher proposed to him. While serving his term in the army he was for a time stationed at a railroad depot, where he amused himself by gradually committing to memory a vast body of statistics relating to timetables, distances between different cities, population, tariffs, etc. When he later took to the stage as a professional calculator, questions based on these statistics formed part of his regular programme. Among his other usual feats are the repetition, either forwards or backwards, of a memorized number of 256 figures, the squaring of numbers up to 4 figures and the cubing of numbers up to 3 figures, finding the 5th powers of 2-figure numbers, and, conversely, extracting the 5th root of any number of 10 figures or less, the cube root of any 9-figure number, and the square root of any number of 7 figures or less, whether the given number is a perfect power or not. In these problems he is aided by his knowledge of many perfect squares, cubes, etc., as well as by various properties of 2-figure endings, with which he is thoroughly familiar. He possibly has a number-form, in which the numbers from 1 to 10, from 10 to 100, and from 100 to 1000 are arranged along three horizontal lines. This number-form, however, if it really exists, plays little or no part in his actual calculations.

Pericles Diamandi[3] (b. 1868), the son of a Greek grain merchant, attributes his calculating gift to his mother, who "has an excellent memory for all sorts of things." One brother and one sister, out of a family of fourteen, share his aptitude for mental arithmetic. He entered school at the age of 7, and remained there until he was 16, always standing at the head of the class in mathematics. But it was only after entering the grain business himself, in 1884, that he discovered his powers of mental calculation, which he now found very useful. He knows five languages,—English, French, German, Roumanian, and his native Greek,—and is a great reader; he has read all he can find on the subject of mental calculation; and he has written novels and poetry, concerning whose quality, however, Binet does not enlighten us. It will thus be seen that Diamandi's education is much better than Inaudi's, and his range of interests correspondingly wider, but that he was far less precocious in calculation than his rival.

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[1] *Rivista sperimentale di Freniatria*, etc., XXIII, 1897, pp. 132-159, 407-429. A summary of these articles, in German, is found in the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, XVI, 1898, p. 314. The writer is indebted to Mrs. Rose Harrington for a translation of considerable portions of the original Italian articles.

[2] Mentally, it is to be presumed, though the article is not explicit on this point.

[3] Binet, *op. cit.*, pp. 110-154, 98, 187 ff. *passim*.

Diamandi is of the visual memory type. He has a number-form of a common variety, running zigzag from left to right, and giving most space to the smaller numbers. This number-form he sees as localized within a peculiar grayish figure, which also serves as a framework for any particular number or other object which he visualizes. He has colored audition for the names of various persons, the days of the week, etc., and if a few figures in a given number differ in color from the rest, he remembers the colors without effort. If the color scheme is more complicated, however, he first memorizes the number and then learns the colors of the individual figures. He always sees numbers as written in his own handwriting, and preferably, if the numbers are large, in a rectangle as nearly square as possible, rather than in one or two long lines. He learns spoken figures (in French) much less readily than written, since in the case of spoken figures he must not only call forth the corresponding visual images, but translate the numbers into his native Greek, in which all his calculations are carried on. Where he seeks to learn the figures very accurately, for purposes of calculation, he is only about half as fast as Inaudi;[1] but where he is concerned with speed rather than accuracy his times are much shorter. In the one case he learned 10 figures in 17 seconds; in the other, 11 figures in 3 seconds.

In calculation Diamandi is considerably slower than Inaudi, whether the numbers concerned are large or small. His time was 127 seconds for a 4-figure multiplication, whereas Inaudi could accomplish the same feat in 21 seconds. Diamandi finds the various figures of the product in order, from right to left, by cross-multiplication; thus in such an example as

$$\begin{array}{r} 46273 \\ 729 \\ \hline 416457 \\ 92546 \\ 323911 \\ \hline 33733017 \end{array}$$

he finds the figures of the partial products not in the horizontal lines of the ordinary method, but in vertical lines,—first

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[1] Here again, however, we must be careful about direct comparisons of dissimilar data, since Diamandi learned from a paper and wrote out his results, while Inaudi depended on audition and speech. Moreover, Diamandi's times were found to be subject to considerable variation from day to day.

7, then 5, 6, then 4, 4, 1, then 6, 5, 1, etc.,—and adds each column before he proceeds to find the numbers that compose the next column. This method has the advantage that the various figures of the partial products can be forgotten almost as fast as obtained, since that figure of the total product which depends on a given column of the partial product is found and recorded as soon as the column is known, and the numbers in that column therefore play no further part in the calculation. On Diamandi's performances in other operations than multiplication Binet gives us no data.

Johann Martin Zacharias Dase[1] (1824-1861) was born in Hamburg. Concerning his heredity we have no information. He attended school at the age of 2½ years, but attributed his powers to later practice and industry rather than to his early instruction. He seems to have been little more than a human calculating machine, able to carry on enormous calculations in his head, but nearly incapable of understanding the principles of mathematics, and of very limited ability outside his chosen field. In this respect he resembled Buxton; but in the rapidity and extent of his calculations he was incomparably superior to Buxton, or indeed to any other calculator on record. He multiplied together mentally two 8-figure numbers in 54 seconds, two 20-figure numbers in 6 minutes, two 40-figure numbers in 40 minutes, and two 100-figure numbers in 8¾ hours; he could extract the square root of a 60-figure number in an "incredibly short time," and the square root of a 100-figure number in 52 minutes. All these times, with the exception of that for the 100-figure multiplication, are probably more rapid, in some cases much more rapid, than those of a good computer using paper. Buxton, it will be remembered, once succeeded in multiplying two 39-figure numbers; other calculators, however, seem to have been unable to handle multiplications much above 15 figures. But if there was any definite limit to Dase's powers, the experiments of which we have record do not show it. We shall later find reason for believing that the 100-figure multiplication was not really a severe tax upon his powers of mental arithmetic. In short, Dase's achievements so far transcend those of any other recorded calculator that he stands in a class by himself, unapproached by any of his rivals.

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[1] Also spelt *Dahse*. The full name is given on the authority of Brockhaus's *Konversations-Lexikon*, ed. 1898, art. *Dase*. Scripture, following the title-page of Dase's posthumously published *Factoren-Tafeln* (3 vols., 1862-5), gives the name as simply Zacharias Dase, which seems to be the way in which Dase usually wrote it. On Dase's life and calculations see Scripture, *op. cit.*, p. 18; *Briefwechsel zwischen Gauss und Schumacher*, Altona, 1861, III, p. 382; V, pp. 30, 32, 277-8, 295-8, 300-304; VI, pp. 27-8, 78, 112; *Crelle's Journal* (*Journal f. d. reine u. angewandte Mathematik*), XXVII, 1844, p. 198; Zacharias Dase, *Factoren-Tafeln*, Hamburg, Vol. I, 1862, Preface; Schröder, *Lexikon d. hamburgischen Schriftsteller*, 1851, art. *Dase;* Preyer, "Counting Unconsciously," *Pop. Sci. Monthly*, XXIX, 1886, p. 221; Brockhaus's *Konversations-Lexikon*, 1898, art. *Dase*. For other references see Scripture, *loc. cit.*

At the age of 15 Dase began his public exhibitions, and continued them for a number of years. He soon numbered among his friends several eminent mathematicians, however, and their influence gradually led him more and more to devote his vast powers to the service of science.[1] Among his (non-mental) computations are included the determination of the value of π to 200 decimal places[2] by the formula

$$\tfrac{\pi}{4} = \tan^{-1}\tfrac{1}{4} + \tan^{-1}\tfrac{1}{8} + \tan^{-1}\tfrac{1}{5},$$

a labor of two months; the computation of the 7-place natural logarithms of the numbers from 1 to 1,005,000; and factor-tables for the 7th and 8th millions (except a small portion) and parts of the 9th and 10th millions. This last task, however, was one in which his patience and perseverance were of more value than his skill in calculation, since, by methods to which Gauss was careful to call his attention, the work was made mainly mechanical. Dase had planned to carry the table through the 10th million, but death cut short his labors. The tables were completed by another hand, and published as far as the 9th million in 1862-5.

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[1] Scripture's statement (*op. cit.*, p. 19) that Colburn and Mondeux "enjoyed even greater advantages [than Dase,] yet failed to yield any results" in the service of science, is misleading. With both Mondeux and Dase the trouble seems to have been not lack of opportunity to acquire mathematical knowledge, but lack of native ability to use the opportunities they had. With Colburn, on the other hand, the trouble really was at least in part lack of opportunity; he certainly did not enjoy the opportunity to attend university lectures, nor did any eminent mathematician try "in vain for six weeks to get the first elements of mathematics into his head" (*ibid.*, p. 18; Gauss-Schumacher *Briefwechsel*, III, p. 382; V, pp. 32, 295), as in the case of Dase. Moreover, Colburn's description of his methods must be reckoned as an important contribution to the science of psychology, none the less important because it is somewhat inferior to Bidder's later description. For other instances of Scripture's unfairness to Colburn, see Appendix I.

[2] Scripture omits to mention any specific number of decimal places, though in both the references he gives (p. 18), to *Crelle's Journal* and to the Gauss-Schumacher *Briefwechsel*, the number of decimal places is made prominent. The natural inference would be that Scripture regarded π as a commensurable number of exactly 200 decimal places; but in view of his frequent use of higher mathematics in his other published works, one hesitates to attribute to him so gross an error. Of course *anybody*, with a logarithm table and a little knowledge of geometry, can compute the value of π to three or four places; the record of such a computation is absolutely meaningless without specific mention of the number of figures to which the computation is carried out.

Dase had one other notable gift, doubtless related to his calculating power: he could count objects with the greatest rapidity. With a single glance he could give the number (up to thirty or thereabouts) of peas in a handful scattered on a table; and the ease and speed with which he could count the number of sheep in a herd, of books in a case, or the like, never failed to amaze the beholder. Here, again, his powers are so far in advance of those of any other recorded person that he stands in a class by himself.

George Parker Bidder[1] (1806-1878), "the elder Bidder," was the son of a stone-mason of Devonshire, England. The indications of hereditary influence are stronger in the Bidder family than in that of any other calculator. Bidder's eldest brother, a Unitarian minister, had an extraordinary memory for Biblical texts, but no special arithmetical gift; another brother was an excellent mathematician and an insurance actuary; a nephew early showed remarkable mechanical ability; Bidder's eldest son, George Parker Bidder, Jr. (hereafter referred to as "the younger Bidder"), inherited in considerable degree his father's gift for mental arithmetic, together with his uncle's mathematical ability, being seventh wrangler at Cambridge in 1858; and two daughters of the younger Bidder showed "more than average, but not extraordinary powers of doing mental arithmetic."[2] Other members of the family were distinguished in non-mathematical ways.

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[1] Scripture, *op. cit.*, p. 23; *Proceedings Institution of Civil Engineers*, XV, 1855-6, p. 251; LVII, 1878-9, p. 294; Colburn's *Memoir*, p. 175; *Phil. Mag.*, XLVII, 1816, p. 314; *Spectator*, LI, 1878, pp. 1634-5; LII, 1879, pp. 47, 111.

[2] *Spectator*, LI, 1878, pp. 1634-5. In this article the younger Bidder is referred to as Mr. G. Bidder; but his full name was the same as that of his father, George Parker Bidder. (*Cf.* Jos. Foster's *Men-at-the-Bar*, 2nd ed., London, 1885, and *The Law List*, London, for 1882.) Scripture refers to both father and son, in different places, as George Bidder, and to the son usually as George Bidder, Q. C., Mr. Bidder, Q. C., or the younger Bidder; by Bidder (unqualified) he always means the elder Bidder, except in one case (p. 28), where the context prevents any misunderstanding. After noting that the similarity of the two names has caused some confusion, he tell us (*loc. cit.*), somewhat dogmatically, that "the only way out of the difficulty is to distinguish the son by adding his title [Q. C.]." (Why would not the son's A. B., or A. M., or his date of birth, or the father's C. E., answer just as well?)

Despite this device for avoiding difficulty, Scripture has fallen into sad confusion in dealing with the various members of the Bidder family. On p. 28 of his article he quotes from the *Spectator* (*loc. cit.*) the sentence: "If I perform a sum mentally, it always proceeds in a visible form in my mind; indeed, I can conceive no other way possible of doing mental arithmetic", omitting the comma after "mentally", but

At the age of 6 Bidder learned from an elder brother to count to 10, then to 100; this was the only formal instruction in figures he ever received. From counting by units to counting by 10's, and then by 5's, was a natural development. He then set about learning the multiplication table up to 10x10, with the aid of shot, marbles, etc., until, as he expresses it, the numbers up to 100 became his friends, and he knew all their relations and acquaintances. A year or so later his readiness in solving simple problems mentioned in his hearing attracted attention, and he acquired a considerable local reputation. Bits of mathematical information (such as that 10 x 100 means 1000, etc.) and halfpence contributed by his admirers conduced to the gradual development of his talent, aided by his natural keenness in reasoning about numerical relations; so that he was soon able to perform 4-, 5-, and 6-figure multiplications mentally. Meantime he came to observe various interesting properties of numbers,—the formulas for the sums of numerous series, casting out the 9's, short cuts in multiplication, properties of squares and of 2-figure endings, and the like. As a

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correctly attributing the remark to the younger Bidder. On p. 57, however, he makes the same quotation, this time adding a superfluous "of" after "conceive" and omitting the comma as before, but now attributing the quotation simply to *Bidder* (unqualified), meaning the *elder* Bidder, as the context unmistakably shows; for a little farther on he says, "This faculty was also inherited [transmitted?], but with a very remarkable difference. The *younger* Bidder [italics mine] thinks of each number in its own definite place in a number-form," etc.

But a worse confusion than this is still to be noted. The *Spectator* correspondence above cited, printed just after the elder Bidder's death, moved another correspondent (*Spectator*, LII, 1879, p. 143) to quote from *Brierley's Journal* for Jan. 25, 1879, the case of an eighteenth century Dissenting minister, the Rev. Thomas Threlkeld, who had a memory for Biblical texts similar to that of the elder Bidder's brother. On the strength of this, Scripture tells us (p. 27): "One of his [the elder Bidder's] brothers was an excellent mathematician and an actuary of the Royal Exchange Life Assurance Office. Rev. Thomas Threlkeld, an elder brother [!], was a Unitarian minister. He was not remarkable as an arithmetician, but he possessed the Bidder memory and showed the Bidder inclination for figures, but lacked the power of rapid calculation. He could quote almost any text in the Bible, and give chapter and verse. [Here Scripture gives the correct reference for this last sentence, which is taken from the younger Bidder's letter, and refers to the brother of the elder Bidder.] He had long collected all the dates he could, not only of historical persons, but of everybody; to know when a person was born or married was a source of gratification to him." Here we are given the correct reference for this last sentence, which refers to the Rev. Thomas Threlkeld, and is from the later volume of the *Spectator*. Thus by a piece of carelessness, hard to excuse, Scripture has inextricably confused the brother of the elder Bidder with this Rev. Thomas Threlkeld, who, so far as we know, was related to the Bidder family only by common descent from Adam!

result of this "natural" algebra and number-theory he hit upon some ingenious methods of performing complex operations; in particular, by his 11th year he was already in possession of a method by which he could solve compound interest problems mentally in an amazingly short time, in fact, almost as rapidly as a good computer using a table of logarithms.[1] Later, after his meeting and competitive test with Zerah Colburn, in 1818, he acquired great skill in the extraction of roots and the finding of factors, by methods similar to Colburn's, but with improvements of his own.[2]

Bidder's reputation soon became more than local, and when about 8 years old he was exhibited in various places by his father, after the fashion so recently set by the Colburns. But Bidder's admirers, more energetic than Colburn's, actually raised a fund to pay for his education, and put him in school. Later on, when his father resumed the profitable exhibitions, friends once more intervened, this time with permanent success. The boy was placed with a private tutor, and in 1819 attended classes in the University of Edinburgh, where he took a mathematical prize in 1822. Leaving the university in 1824, he held positions successively in the Ordnance Survey and in an assurance office. But by the advice of his friends he later decided to devote himself to civil engineering, and ultimately became one of the most successful engineers of his time. He was connected with several engineering undertakings of the first magnitude, and as a member of the Institution of Civil Engineers took a prominent part in the controversies then before the profession. Constant use kept up his calculating powers, and in various railway and other contests before Parliamentary committees his great command of statistics and keen powers of analysis made him a formidable witness.

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[1] On the mathematical side, if P represents the principal, r the interest (as a fraction of the principal, not as a per cent.), and n the number of years, Bidder's method amounted to the expansion of the expression $P(1+r)^n$, by the binomial theorem, to a sufficient number of terms to insure accuracy in the last farthing. The properties of several numerical series were skillfully utilized at different stages of the expansion. (*Cf. Proc. Inst. C. E.*, XV, p. 267, for Bidder's own account.)

[2] Colburn says of this meeting (*Memoir*, p. 175), "Some time in 1818, Zerah was invited to a certain place, where he found a number of persons questioning the Devonshire boy. He [Bidder] displayed great strength and power of mind in the higher branches of arithmetic; he could answer some questions that the American would not like to undertake; but he was unable to extract the roots, and find the factors of numbers." Thus it would seem that Bidder's mind was not strongly turned in the direction of this class of problems until after this meeting with Colburn, but that once he became interested in them he soon outstripped his rival. Strangely enough Scripture, after mentioning this passage from the *Memoir* in his general bibliography on Bidder, does not cite it in his account of the meeting of Colburn and Bidder, but refers only to the one-sided account of a London paper, which represents Bidder's triumph as complete. For a further discussion of this meeting, see Appendix I.

It would seem that Bidder's powers of mental calculation increased steadily at least up to the beginning of his university days, if not later,[1] and thereafter remained almost undiminished to the end of his life. Both in numerical calculations and in his study of higher mathematics he was interested in general principles, practical applications, and striking properties, rather than in intricate analysis for its own sake, or calculations with numbers chosen merely for their length. At Edinburgh he maintained a good class standing in mathematics, including differential and integral calculus, but only by hard study.[2] In the solution of problems where special properties or symmetries played a part he was equalled, if at all, only by such great calculator-mathematicians as Gauss and Ampère. In division his skill was considerable. In multiplication he was able, on one occasion, to handle two 12-figure numbers, but only by "a great and distressing effort";[3] in general, he did not cultivate his calculating power much beyond the limits of its practical usefulness to him. In his lecture "On Mental Calculation," before the Institution of Civil Engineers,[1] to which reference has already been made, Bidder has left us an excellent account of his methods of calculation.

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[1] In the *Spectator*, LII, 1879, pp. 111-112, are given specimens of Bidder's feats during the years 1816-1819, with times of solution, also the London newspaper account of his meeting with Colburn to which reference has already been made. Scripture (*op. cit.*, p. 26) argues from this series that Bidder's powers increased between 1816 and 1819. That this was the case can hardly be doubted; but it certainly is not proved *by this series of examples*. Even comparative times for an expert computer solving these same problems on paper would prove nothing, since in several cases there are two or three different ways of doing the work, and possible short cuts which it is impossible to say whether Bidder noticed or not. Moreover, no two of the problems are alike. Perhaps the hardest problem of the lot is the compound interest question (1816, solved in 2 minutes) which is first in the list. The cube root of the 18-figure number (1819, 2 minutes) is far easier than it looks; for by this time, a year after his meeting with Colburn, Bidder was doubtless familiar with the application of 2-figure endings to these problems, so that he had only to find the cube root of the first 9 figures by trial and approximation to get the first three figures of the root, then add on the last two by inspection from the last 2 figures of the given number, and find the missing 4th figure of the root by casting out the 9's. The algebraic problem which was solved "instantly" in 1819 was very simple, and was undoubtedly solved by inspection; the answer, 3, was, from the nature of the question, the most natural first trial, and hence no special credit belongs to this last feat. These considerations show how difficult it is to reach definite conclusions from particular problems of this sort unless there is at hand specific knowledge of the detailed methods and short-cuts actually used in the examples under consideration, particularly of any special peculiarities of the given numbers whereby the solution may be facilitated.

[2] *Proc. Inst. C. E.*, XV, p. 253; *Spectator*, LI, 1878, pp. 1634-5.

[3] *Proc. Inst. C. E.*, XV, p. 259. In view of this explicit statement

George Parker Bidder, Jr.[2] (b. 1837), "the younger Bidder," was the eldest son of G. P. Bidder. Practically the only information we have concerning his powers of calculation consists of a few facts brought out in the *Spectator* correspondence already referred to. He was 7th wrangler of his year, and later a thriving barrister and Queen's Counsel. He tells us that he was unable to approach his father in extent of memory and rapidity and accuracy of calculation; we have seen, however, that the father, writing in his 50th year (after which his powers can hardly have shown any considerable increase), speaks only of multiplying 12 figures by 12 figures "on one occasion", by "a great and distressing effort", whereas the son was able, in several instances, to perform 15-figure multiplications, though slowly and with occasional errors. That the younger Bidder's method of multiplication was, like Diamandi's, cross-multiplication, we may infer from the fact that he incorrectly attributed this method to his father. Of the son's other feats in calculation, and of the degree of his precocity in this field, we have no knowledge. He was of visual memory type, and possessed a number-form running from right to left, the numbers up to 12 being arranged in a circle as on a clock. He declared that his calculations "proceed in a visible form" in his mind, and that he "can conceive no other way possible of doing mental arithmetic," which, as Proctor points out,[3] is a rather strange remark. Unlike most of the other calculators, he employed a mnemonic system instead of natural memory in remembering numbers. He could play two games of chess blindfold simultaneously.

Truman Henry Safford[4] (1836-1901) was, like Zerah Colburn, the son of a Vermont farmer; but both his parents were

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from Bidder himself, his son and Elliot seem to be wrong in attributing to him (*Spectator*, 1878, p. 1634) great facility in 15-figure multiplications. The son's statement that his father used cross-multiplication is likewise at variance with the father's explicit account of his method of multiplication (*Proc.*, XV, p. 260).

[1] *Proceedings*, XV, pp. 251 f.

[2] Referred to by Scripture as George Bidder, Q. C. Scripture, *op. cit.*, p. 28; *Spectator*, LI, 1878, pp. 1634-5; Galton, *Inquiries into Human Faculty*, pp. 133-4, and Plate I, 20, opp. p. 380.

[3] *Belgravia*, XXXVIII, 1879, p. 461.

[4] Scripture, *op. cit.*, p. 29; Appleton's *Cyclo. of Am. Biog.*, art. *Safford; Chambers's Edinburgh Journal*, N. S. VIII, 1847, p. 265; *Belgravia*, XXXVIII, 1879, p. 456.

former school-teachers, and persons of some education. The father had a strong interest in mathematics, and the mother, we are told, was of an "exquisite nervous temperament." Young Safford showed a remarkable all-round precocity, similar to that of Ampère. In his 3rd year "the grand bias of his mind was suspected"; later his parents "amused themselves with his power of calculating numbers"; and when he was 6 years old he was able to calculate mentally the number of barleycorns, 617,760, in 1040 rods. At the age of 7 he had "gone to the extent of the famous Zerah Colburn's powers." About this time he began to study books on algebra and geometry, and soon afterwards higher mathematics and astronomy. Wanting some logarithms, he found them himself by the formulas; and in his 10th year he published an almanac computed entirely by himself. The following year he published four almanacs, one of which, computed for Cincinnati, at once reached a sale of 24,000 copies. In this almanac he used a new and original rule for obtaining moon risings and settings, accompanied by a table which saved a quarter of the work of their computation. About this time he also discovered a new rule for calculating eclipses, with a saving of one-third in the labor of computing.

Such feats at once made the boy a public character, and in the same year (1846) he was examined by the Rev. H. W. Adams, a skillful mathematician. He solved a number of difficult algebraic problems, doubtless in the main by algebraic methods rather than by the trial and error method of most of the other prodigies. Problems in the mensuration of solids caused him no trouble, though in one case, where the answer was a 12-figure number, he "used a few [written] figures." He extracted the cube roots of 7-figure exact cubes "instantly," doubtless by the use of 2-figure endings. Finally, he squared 365,365,365,365,365,365, entirely in his head, in "not more than one minute,"[1] though with evident effort. A three-hour examination convinced Adams that the boy had mastered and gone beyond all his text-books.

Like Ampère, Safford had a wide range of interests, and an encyclopedic memory. Chemistry, botany, philosophy, geography, and history, as well as mathematics and astronomy, were included in his field of study. He took his degree at Harvard in 1854, and became an astronomer. After holding various positions he became professor of astronomy in Williams College in 1876, where he remained until his death in 1901.

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[1] All these quotations are from the *Chambers's Journal* article cited above. The last problem is there given as 365,365,365,365,365 x 365, 365,365,365,365,365, *i. e.*, a 15-figure number multiplied by an 18-figure number; but since the answer contains 36 figures, it is obvious that another 365 is omitted from the first number, and that the problem was the squaring of an 18-figure number. The repetition of the same figures, however, greatly simplified the work, there being only three different partial products. Scripture carries over the typographical error without comment, evidently not noticing it.

Safford early outstripped Bidder in range of mental calculation, but with the aid of books, whereas Bidder's methods were entirely of his own discovery. It is to be regretted that we have not more detailed information about Safford's calculations;[1] but except for the examination whose results have been given above, all we can say is that later he acquired considerable skill in factoring large numbers, seeming to be able to recognize almost at a glance what numbers were likely to divide any given number, and remembering the divisors of any number he had once examined.[2]

André Marie Ampère[3] (1775-1836), like his successor Safford, showed all-round precocity, a wide range of interests, and an omnivorous memory. He learned counting at the age of 3 or 4, by means of pebbles, and was so fond of this diversion that he used for purposes of calculation pieces of a biscuit given him after three days' strict diet. He became a noted mathematician, and was also prominent in several other directions. Of his mental calculations, however, we have no specific information; his later achievements so overshadowed his early gift that his biographers are silent about it, and his case sheds little light on the problems connected with the subject.

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[1] The *Chambers's Journal* article is written in rather florid style, and in a tone of admiration almost verging on awe. The Rev. H. W. Adams, who is there said to have been a skillful mathematician, was by no means as critical an examiner as might be wished. Thus while he tells us that several of the problems given were among the hardest in Davies' *Algebra*, he later notes that Safford already owned this work and had fully mastered it, hence had seen all these problems before. The times indicate, to be sure, that Safford calculated the answers afresh; but the test is not as satisfactory as if the problems had been entirely new to him. The times given, too, are mostly in the form "about a minute," and in definiteness leave much to be desired. The big number selected for the grand final test was about as unsuitable for the purpose as any that could well have been chosen. Not only in the recurrence of the same three partial products, but in the repetition of the same group of figures within each partial product, the problem is so artificially simple that it proves almost nothing concerning Safford's power of multiplication. The number 365, too, owing to its connection with the calendar, is especially easy to remember. Adams speaks of the "long and blind sums" which Safford remembered after a single hearing; but apart from this simple 18-figure number (which would not overtax the memory of any child who could keep in mind 365 and count six), the longest numbers in the statement of any of the problems mentioned in the article were of 7 figures. Now a normal boy of 13 can, on the average, retain 8.8 figures after a single hearing, and a boy of 11, 6.5 figures. Hence, while Safford's memory for figures was probably above the average, the fact is not satisfactorily proved by Adams' examination. (*Cf. American Journal of Psychology*, II, 1889, p. 607. The figures are erroneously quoted by Scripture, p. 41, as 8.6 for boys of 19 years, instead of 8.8 for boys of 13 years.) The fact that the *answers* to some of the problems were longer numbers is not relevant here; for, as we shall see later, there is an important distinction between figure-memory as such, and memory as it stands *in the service of calculation*.

[2] *Belgravia*, XXXVIII, 1879, p. 456.

[3] Scripture, *op. cit.*, p. 6; Arago's Éloge d'Ampère, tr. in *Smithsonian Report*, I, 1872, p. 111. The writer has been unable to consult the other references which Scripture cites.

Carl Friedrich Gauss[1] (1777-1855) was the son of a poor family; a maternal uncle of his, however, was a man of considerable mathematical and mechanical talent. When not quite 3 years old, Gauss, according to an anecdote told by himself, followed mentally a calculation of his father's relative to the wages of some of his workmen, and detected a mistake in the amount. Entering the gymnasium at the age of 11, he mastered the classical languages with incredible rapidity. In mathematics he was not only head of the class, but soon outstripped his teachers. At the age of 10 he was ready to begin the study of higher analysis, and at 14 he could read the works of Euler, Lagrange, and Newton. He became one of the foremost mathematicians of his time. His *Disquisitiones Arithmeticæ*, published at the age of 24, is practically the foundation of the modern theory of numbers.

Concerning Gauss' mental calculations we have for the most part only general information. His power seems to have lasted all his life, and to have exceeded that of any other calculator except Dase. He had a "peculiar sense for the quick apprehension of the most complicated relations of numbers," and "an unsurpassed memory for figures," and used from memory the first decimals of logarithms in his mental operations. He was especially fertile in inventing new artifices and methods of solution.

MINOR PRODIGIES.—In the following list are grouped a few calculators about whom too little is known for an extended account, but who present one or more points of interest.

The Daughter of the Countess of Mansfield[2] (b. about 1804) was seen by Spurzheim in London at the age of 13, at which time she "extracted with great facility the square and cube roots of numbers of nine places." Whether this refers only to *perfect* squares and cubes cannot be decided. Colburn speaks of her simply as displaying, in 1812, at the age of 8 or thereabouts, "a certain degree of mental quickness [in calculation] uncommon in her sex and years." Except for Bidder's two granddaughters, whose powers were but little above the average, she is the only girl calculator on record.

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[1] The writer has followed Scripture's account of Gauss (*op. cit.*, p. 7), not having access to the sources there cited.

[2] Gall, *op. cit.*, V, p. 88; Colburn, *Memoir*, p. 174; Scripture, *op. cit.*, p. 32. The reference to the *Med. and Philos. Jl. and Rev.* given by Scripture can hardly be correct, since the young lady, being about Colburn's age, was in 1811 only 6 or 7 years old, and could hardly have had an American reputation. The exact words in Scripture's text are found in Gall's *Organology;* the *Jl. and Rev.* reference probably refers to Mr. Van R., of Utica. In fact, all the notes to page 32 of Scripture's article are incorrect except a few of those to Gall, where the absence of a page reference covers up the inaccuracy. The trou-

Richard Whately[1] (1787-1863) began to calculate at the age of 5, and retained the power for about three years; he probably surpassed Colburn, but did not happen to hit on Colburn's favorite problem of extracting square and cube roots. When he went to school the power left him, and at ciphering he was always "a perfect dunce."

Mr. Van R., of Utica,[2] like Whately, developed a gift for calculation at an early age (6 years), but lost it at the age of 8.

Dr. Ferrol[3] (b. 1864) has a sister about a year his elder, who shares his gift for mental calculation. His father was an architect and a good reckoner, and his mother's mind was occupied with architectural computations at the time of the birth of these two children; whether this prenatal influence had any effect on their mental powers cannot be determined. Ferrol's gift showed itself at an early age, but as soon as he learned the elements of algebra, at the age of 10, he developed a preference for mental algebra instead of mental arithmetic. He was head of his class in mental arithmetic, but below the average in all other studies. He is a remarkably poor visualizer. His processes are "intuitive"; the answer to a problem, he tells us, comes "instantly," and is always correct. His general memory is probably about normal; his figure memory depends on mnemonics.

A blind Swiss mentioned by Johannes Huber[4] not only solved

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ble seems to be due to a transposition; note [1] should be note [7] and all the others should be moved up a line, [2] becoming [1], etc. Colburn's account of the daughter of the Countess of Mansfield is quoted in full in Appendix I.

[1] Scripture, *op. cit.*, p. 10. The writer has been unable to consult the *Life* of Whately there cited. By an inadvertance, Scripture, on p. 57, gives the age of Whately's first calculations as 3, whereas, on p. 10, the statement is "between five and six."

[2] Gall, *op. cit.*, pp. 87-8, quoting *Med. and Philos. Jl. and Rev.*, III, N. Y., 1811. Gall mentions several other calculators, but it has not seemed worth while to enumerate them all here.

[3] P. J. Möbius, *Die Anlage zur Mathematik*, 1900, p. 73. The name is given simply as "Dr. Ferrol"; we are not told whether he is an M. D., or what are his initials.

[4] *Das Gedächtniss*, Munich, 1878, p. 43.

the most difficult problems, but could repeat a series of 150 figures either forwards or backwards after a single hearing, or name at once the 30th or 50th figure, *e. g.*, from either end. Before becoming blind he had been a man of very weak memory; but afterwards, busying himself with exercises in calculation, he discovered a very simple method of dealing with the largest numbers, and tried to sell his secret in England for a high price.[1]

Vito Mangiamele[2] (b. 1827), the son of a Sicilian shepherd, himself tended sheep, and when examined by the Academie des Sciences, at the age of 10, answered several questions, among them the cube root of 3,796,416 (=156), which he found in half a minute. Cauchy, in his Academie report on Mondeux, already cited, complains that Mangiamele's masters have always kept secret the boy's methods of calculation; it is not clear whether this means that they knew and refused to tell, or that the boy himself was unable to enlighten them. He was quite uneducated. A brother and a sister of his were also noted calculators.

Prolongeau[3] (b. about 1838), at the age of 6½, solved mentally with great facility problems relating to the ordinary operations of arithmetic, and to the solution of equations of the first degree.

Grandmange[4] (b. about 1836), born without arms or legs, performed, mentally, very complicated calculations and solved difficult problems.

Mathieu le Coq[5] (b. about 1656), an Italian boy, "at the age of 6, without knowing how to read or write, commenced to perform all the most difficult operations of arithmetic, such as the four elementary operations, the rule of three, partnership (*compagnie*), square and cube root, and that, too, as soon as the question was put to him." He learned to calculate by stringing beads.

Vincenzo Succaro[6] (b. 1822), a Sicilian, appeared in public as a calculator at the age of 6, received a good education, but showed no special mental ability outside of calculation.

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[1] Euler, it is well known, possessed considerable powers of mental calculation after becoming blind; but to what extent he had the power before his blindness, and just what feats he could perform, the writer has been unable to discover.

[2] *Comptes rendus hebdomadaires des séances de l'Academie des Sciences*, IV, 1837, p. 978; *Riv. sper. di Fren.*, XXIII, 1897, p. 434.

[3] *C. R. Acad. des Sci.*, XX., 1845, p. 1629.

[4] *Ibid.*, XXXIV, 1852, p. 371.

[5] Binet, *op. cit.*, p. 3; *Riv. sper. di Fren.*, XXIII, 1897, p. 430.

[6] The source for the remaining calculators is the *Riv. sper. di Fren.*, XXIII, 1897, pp. 429 f. A summary of this article in German is found in the *Zeits. f. Psy. u. Physiol. d. Sinnesorgane*, XVI, 1898, pp. 317-8.

Giuseppe Pugliese (b. "a little later"), also a Sicilian, took to the stage at the age of 5, and was exhibited in Italy and Germany. An attempt was made to teach him Geometry; but he was unable to deal with geometrical forms.

Luigi Pierini (b. 1878) learned late to speak and to walk, suffered from many children's diseases, and was an epileptic. He tended sheep, and thus learned to count. He developed a remarkable talent for mental arithmetic, and at an early age became a professional calculator.

II.

The writer will now give an account of his own case, which differs in three respects from those hitherto considered:

(1) The power is almost confined to dealing with the last two figures, or 2-figure endings, of the numbers used. It is readily seen that, with certain limitations in division and evolution, the last two (or n) figures of the numbers used in a given problem determine the last two (or n) figures of the answer, no matter what the preceding figures may be. Now the writer's mental calculations take the form almost exclusively of tracing the last two figures through the different operations, ignoring all the other figures. This evidently simplifies the work immensely.

(2) By a further specialization, the problems which he solves most often and most readily are of the general form of finding the last two figures of any power (or integer root) of any number.

(3) Finally, he has a strongly marked preference for working with even numbers. By a special method, to be explained later, he practically always changes odd numbers into even numbers for purposes of calculation, where only the last two figures of the answer are required; the even number thus obtained is readily converted into the desired odd number by very simple rules.

It will thus be seen that the writer's calculations are highly specialized, and in extent perhaps not comparable to those of any calculator heretofore considered. At the same time, some of these specializations are found in other calculators; and in the general features of its development the writer's case is typical of many or most of the others, and will, it is hoped, throw light on several points which have hitherto not been fully understood. While many of the details are in themselves of little importance, they will serve to illustrate the sort of numerical properties which not only facilitate mental calculation, but arouse the interest of the calculator, and hence furnish the motive for continued practice until the calculating habit becomes firmly established.

In the matter of heredity the only circumstance that need be mentioned is that the writer's younger brother has shown rather more than average ability as a chess-player, and has, on a few occasions, played a game blindfold; but by what psychological processes, or to what extent the power could be increased by further practice, cannot be stated. Nor need the writer speak of his school and college work, except to say that while he has always been fond of mathematics, it has no better claim than two or three other subjects to be called his favorite study.

His interest in mental calculation dates from the time he learned to count, at the age of 4, or possibly 3.[1] He learned to count to 10, then to 100, then beyond, and also to count by 2's, 3's, etc. Now in these latter series 2x2, 2x2x2, 3x3, 3x3x3, etc., in short, the powers of the number by which he was counting, were natural resting-places, and awakened his interest, so that before long he began to count in the power series of different numbers (2, 4, 8, 16, 32, etc., 3, 9, 27, 81, etc.) for considerable distances. At first he simply emphasized the powers as they occurred in the complete series of multiples, but gradually he learned to omit the intermediate multiples, and simply count in the power series proper: 2, 4, 8, 16, etc., 3, 9, 27, 81, etc. But almost always, when the number exceeded 100, he emphasized the last two figures, and gradually got into the habit of ignoring all the others. Thus instead of saying 3, 9, 27, 81, 243, 729, 2187, etc., he usually counted 3, 9, 27, 81, 43, 29, 87, and in this simplified form counted along the different power series for considerable distances. Multiplication naturally grew out of this counting process; but it was really *counting* rather than multiplication proper, since he did not learn the multiplication table until some time later, when he went to school. Thus to find 9x7 at this time he would count 9, 18, etc., to 63; and even now, except within the limits of the multiplication table as he learned it to 12x12, his mental multiplications are abbreviated countings of this sort (skipping most of the intermediate links) rather than true multiplications. We have already seen reason to suspect that neither Buxton nor Tom Fuller really got beyond this counting process into true multiplication, *i. e.*, with the use of a memorized multiplication table.

In the course of these calculations or countings, a number of properties gradually attracted the writer's attention; such as that every power of a number ending with 0 or 5 ends with 0 or 5, that the 4th power of any other number ends with 1 or 6, according as it is odd or even, that the 5th power ends with the same figure as the 1st, the 6th with the same figure as the 2nd, etc.; and that if 76, or any number ending with 76, is multiplied by a multiple of 4, the last two figures of the product are the same as those of the multiplier (*e. g.*, 76x12=912). Then he noticed that the ending[1] 76 occurs at various points in the power series of different numbers (the 5th power of 6, the 4th power of 32, the 2nd power of 24, the 10th power of 4, the 20th power of 2, etc.,), and that from these points the series of endings repeats, except that in some cases the ending of the next power will differ by 50 from that of the original number. Thus the endings of the first 20 powers of 2 are 02, 04, 08, 16, 32, 64, 28, 56, 12, 24, 48, 96, 92, 84, 68, 36, 72, 44, 88, 76; the 21st is 52 instead of 02; but the 22nd is 04, like the 2nd, and thereafter the endings recur in regular order. Finally it turned out that the *20th* power of every even number (not ending with 0) had the ending 76, and that odd numbers had a similar property, the 20th power ending being, however, 01 instead of 76, and even the 21st power being always the same as the 1st, except for multiples of 5.

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[1] Unfortunately, definite dates cannot be given. The power developed very slowly, never really becoming important for any but psychological purposes, so that no one but the writer himself knew of its existence until a much later date.

After discovering these and similar properties, the writer found it a simple matter to find the last two figures of any power of any number, by counting along the proper series. The process was always, however, of the counting type already indicated. Thus to find the 8th power of 3 the process would be 3, ^{6}, 9, ^{18}, 27, ^{54}, 81, ^{62}, 43, ^{86}, 29, ^{58}, 87, ^{74}, 61; *i. e.*, he would count up to a power of 3, then by this power to the next, and so on, but passing very lightly over the intervening multiples, and in time learning to omit them altogether. In fact, before long the process came to be simply, *3*, 9, 81, *61*, *i. e.*, simply squaring each number to get the next, the intermediate countings taking place so rapidly and automatically as hardly to appear in consciousness at all, except as brief "flashes." And even these "flashes" may sometimes be almost absent, so that only the 3 and the 61 stand out, the rest remaining a mere blur.

It happened that about the time he learned to count, and for perhaps two or three years thereafter, the writer was frequently ill. This, of course, left a large amount of time free for his calculating exercises, and probably had not a little to do with strengthening his bent in that direction.

We come now to the third peculiarity mentioned above: the writer's preference for even numbers. An examination of the following table of the endings of certain products will form the best introduction to this subject.

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[1] The term "ending" (unqualified) will hereafter be taken as synonymous with "2-figure ending."

	07	32	57	82
23	61	36	11	86
48	36	36	36	36
73	11	36	61	86
98	86	36	86	36

It will be observed that the numbers at the left, 23, 48, 73, 98, differ in pairs by 25, and so with the numbers at the top, 07, 32, 57, 82; and that in each case there is one multiple of 4 (48, 32), one odd multiple of 2 (98, 82), one number of the form $4c+1$ (73, 57), and one number of the form $4c-1$ (23, 07). Now the 16 numbers in the body of the table, it will be seen, all belong to a similar series, 11, 36, 61, 86. If either of the factors is a multiple of 4, the product has the ending 36, as shown by the 2nd line and the 2nd column; if both are odd multiples of 2 (98, 82), the product again ends with 36; if one is an odd multiple of 2 (98, 82), and the other an odd number, the product has the ending 86, $=36+50$. Finally, if both numbers are odd, the ending of the product is 36 ± 25, *i. e.*, either 11 or 61:- 61 (a number of the form $4c+1$) if the numbers multiplied are either both of the form $4c+1$ (73×57), or both of the form $4c-1$ (23×07); and 11 (a number of the form $4c-1$) if one of the factors is of the form $4c+1$ and the other of the form $4c-1$ (73×07, 23×57). Thus by applying a few simple rules, any one of the 16 products in the table can be made to depend on the single product, 48×32, of the two multiples of 4 in the table. Hence to find the ending of the product of two odd numbers, change each into a multiple of 4 by adding or subtracting 25, multiply these multiples of 4 together, and then add or subtract 25, as the case may require, to get the answer. A similar principle obviously applies to the power series of any odd number; simply find the required power of the corresponding even number, and then either add or subtract 25.

Now these properties early attracted the writer's attention, and he soon got into the habit of transforming odd numbers into even numbers in practically all his calculations. The result was that (if we leave out of account multiples of 5, which belong to a class by themselves and are very easy to multiply) the whole of multiplication, so far as the endings were concerned, was reduced to the 200 possible products of any two of the 20 numbers 04, 08, 12, 16, 24, 28, 32, 36, etc.; whereas in order to do the same work without this transformation, the 3200 combinations of the whole eighty 2-figure endings prime to 5 would have to be considered. In finding powers, again, he had to deal with only 20 different series, each of which repeated after 20 terms or less; so that the whole problem of finding the last two figures of any power of any number was reduced to less than 400 simple cases, instead of an indefinite number of cases. He never committed these products and powers to memory; it was not necessary; with practice he was soon able to count to any desired one with great rapidity, in fact, just as rapidly, in the simpler cases, as he could have recalled the answer if it had been previously memorized.

To recapitulate: The writer's mental calculations usually deal only with the 2-figure endings of numbers, rejecting all previous figures if there are any; by far the commonest problem is to find (the ending of) some given power of a given number, or to investigate some property of some power or group of powers of one or more numbers; and problems involving odd numbers (except, of course, odd exponents) are almost always solved by changing the odd numbers into multiples of 4 (by adding or subtracting 25), and changing back to an odd number in the same way, if necessary, after the work of calculation is over. He might go on and indicate many other properties of numbers, or rather of endings, which he discovered and used in calculating; but enough has already been said to give a fair idea of the general nature of the processes employed, the gradual development of the calculating power, and the advantages of the various specializations which came to be adopted.

Of course his calculations are not absolutely confined within these limits. Besides finding endings in the power series of even numbers, he can also multiply endings very readily, and add or subtract them (by counting forwards or backwards) somewhat less rapidly, or divide them where the division is known to be exact; and he *can* work, though very much more slowly, with odd numbers. But even in the power series of 3, the odd series with which he has worked oftenest, it is easier in most cases to change 3 into 28; and in any other odd series he can scarcely work at all, except with the greatest effort. The even series in these other cases are so much easier and more familiar that it is practically impossible to resist the temptation to work in them, even when he tries to work laboriously in the odd ones as such.

When the calculation takes account of *all* figures of the result, not merely of the last two, the writer's powers of mental arithmetic are probably very little above the average, certainly not equal to those of any one who has had a moderate amount of practice in the work. Even the multiplication of two 2-figure numbers takes him longer mentally than on paper; and with 3-figure numbers it is such an effort for him to remember the partial products that usually each one must be repeated aloud two or three times, and even then he is apt to forget the first partial product by the time he has found the third. With small 2-figure numbers, however, he finds no difficulty in multiplying (on paper, using only one figure of the *multiplicand* at a time,) in a single operation, especially where the number is even, *e. g.*, 24 or 36. With 19 or 23, too, it would probably be easier for him to multiply in a single operation than in two operations in the ordinary way; but in such a case, after the products exceeded 100, the multiplication would often tend to resolve itself into counting,—rapid and automatic, but counting nevertheless. Thus up to 23x5 = 115 he would probably count by 23 directly, or depend on his memory; but after that, to pass to 23 x 6 = 138, he would first count in the 3, then the 20, thus reaching 138 from 115 *via* 118 and 128.

There are two cases in which the writer can find complete products with fair readiness. The first is in squaring numbers; here, however, the process is usually neither counting nor multiplication directly, but an application of some algebraic formula. Up to perhaps 32, and in certain other cases, such as 36, 48, 54, 64, 72, 81, 96, 144 (*i. e.*, numbers containing no other prime factors than 2 and 3), he would give the squares from memory; but usually he finds only the last two figures by memory, and gets the rest by interpolation between two known squares or by the formula for $(a+b)^2$.

The second case is where two numbers are to be multiplied, neither of which contains any prime factors except 2 and 3. Here his method is to count (multiply) by 2's or 3's to some convenient multiple of one of the numbers, then by that multiple to some other, and so on, until the required product is reached. Thus to find 48 x 64 he would count by 48 to 384 (=48 x 8), then by 384 to 1536, then to 3072 (= 384 x 8 = 48 x 64), the required answer. To square 162, again, the stages would be 486, 1458, 2916, 8748, 26244, *i. e.*, multiplying successively by 3, 3, 2, 3, 3.[1] In these cases much of the work would be automatic and half-unconscious. Thus up to 2916 $(=162 \times 18 = 54^2)$ in the second example the numbers in full would be very familiar, and perhaps only the 58 of 1458 would be distinctly formulated; but above that he would have to formulate all the figures distinctly and take account of them in the counting process. In the first example, 48 x 64, perhaps only the 84 of 384 and the 36 of 1536 would be distinctly formulated until the end, when 3072 would be given as a whole. It will be seen, then, that part or all of the intermediate numbers of the calculation may remain below the level of clear consciousness, and that where the numbers are familiar, part of a number may be in clear consciousness and not the rest of the number. At the same time the whole number functions in the calculation, otherwise the correct answer would not result.

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[1] Buxton, it will be remembered, in multiplying 456 by 378, multiplied successively by 5, 20, and 3, to get 300 x 456; then multiplied 456 x 5 and that product by 15, and added the result to 300 x 456, to get 375 x 456; and finally completed the operation by adding 3 x 456. This indicates pretty clearly that his method was like the one described above, a counting in the series of multiples of the multiplicand, rather than the ordinary method.

There is just one class of problems in which the writer could compete with the real mathematical prodigies, *viz.*, finding the square and cube roots of exact squares and cubes. In fact, extracting the roots of perfect powers and testing the possible factors of given numbers are the only fields in which the properties of 2-figure endings are really useful, and even these problems, however interesting to the mathematical prodigy, are of little practical importance to the mathematician. Bidder, Colburn, and Safford made a specialty of these problems, and there is good evidence that all three solved them by the aid of the properties of 2-figure endings. A brief description of the "method of endings" will therefore not be out of place.

Given the last two figures of a number, the last two figures of its square are known; but given only the last two figures of a perfect square, the last two figures of the square root are not definitely known, although the possible values are usually only four in number. Similarly, an odd ending has only one possible cube root, but an even ending has either none, or two which differ from each other by 50. Now, suppose a given number is known or suspected to be a perfect square or cube, and its root contains only three figures. The first figure can readily be determined by inspection; and the last two figures must be one of a limited number of possible roots of the ending of the given number. It is usually easy, after a little practice, to tell almost at a glance which of the possible roots to choose in a given case. In doubtful cases (multiples of 5, *e. g.*, where the number of possible roots is greater) such expedients as casting out the 9's, squaring or cubing one of the suspected answers or some number near it, or using the 3-figure instead of the 2-figure ending, will help to decide which is the correct root.

The application to factoring is still simpler. If the number to be factored is not already odd and prime to 3 and 5, it is easily made so by simple division. Now, in the case of an odd number prime to 5, if the last two figures of one of its divisors are known, and the division is exact, the last two figures of the other can have only one value; and it is easy to construct a table showing the different pairs of endings in the factors which will produce a given ending in the product. Now, suppose it is suspected that a given number is a factor. From the table, or by a computation in accordance with simple rules, which need not be considered here, find the last two figures of the other factor; if desired, the hundreds figure can also be determined by casting out the 9's. This done, it is necessary to carry the division only far enough to decide whether the required last two or last three figures can result; as soon as this is seen to be impossible, the work is abandoned, since only an exact divisor is wanted. It is thus evident that much work may be saved, especially where the numbers involved are not very large; indeed, a factor may often be rejected almost at a glance which would otherwise have to be divided through to the end.

So much for the application of 2-figure endings to evolution and to factoring. The latter problem never attracted the writer, owing to the habit he so early developed of confining his attention to the last two figures; but in any case where a given number is known to be a perfect square or cube, and its root contains not more than three figures, he finds no difficulty in discovering the root by inspection. This would apply almost equally to higher roots, except that in some cases it would be difficult to tell the root if it contained more than two figures; but in general, the higher the root the easier the problem, and square and cube roots are the only ones which often come up. It is evident, however, that skill in solving this class of problems does not imply special skill or quickness in other branches of mental arithmetic, and that a careful distinction must be made between the cases where the given number is a perfect power and those where it is not. Where the root is not an integer, the ending gives no aid in finding it; memorization of a large number of perfect squares and cubes, or some process of real calculation, must then be resorted to, instead of the simple method of guessing by inspection of the ending of the given number.

Before closing this part of the paper, the writer may say a few words about his memory type. He learned to count orally, and his calculations began at once, without further aid; he cannot remember ever counting on his fingers, using pebbles, or the like; and even when he learned to make written figures later on, they never came to be associated with his mental calculations, which remained strictly auditory (or auditory-motor) throughout. Ordinarily the motor element is almost entirely absent; when the calculations remain in the familiar fields already described, they are accompanied by no perceptible innervation of the muscles of speech. When he attempts unpracticed feats, however, such as complete 3-figure multiplications, the tendency to pronounce some or all of the figures is marked.[1]

But while the writer's type is unquestionably auditory in calculation, the presence of written figures is not a hindrance to him, as it is to Inaudi. On the contrary, if the numbers involved are at all large,—say a 9-figure number whose cube root is to be found,—the presence of the number on a sheet of paper before him is a distinct aid, saving a considerable effort of memory, and greatly facilitating such tests as casting out the 9's. Outside of calculation the writer's type is predominantly auditory; but he can use visual images at will with no special difficulty, and in geometry or similar fields uses them habitually as a matter of course. In general, then, his type is mixed, but with a slight predominance of auditory images.

It only remains to add that his calculating powers have increased, though very gradually, from the time he learned to count until the present, constantly taking advantage of the results of his mathematical studies, and at intervals following out new lines of inquiry and classes of problems based upon new properties of numbers and endings. There has been no tendency, however, to enter the broader fields of calculation cultivated by the mathematical prodigies; in the main, his calculations are confined within the limits already described, and even within these limits it often happens that of two problems which, to an ordinary calculator, would be of equal difficulty, one will be far easier for him than the other, owing to the peculiar preferences which have guided the distribution of his practice in calculation. While mental arithmetic has never absorbed a disproportionate share of his time, there is scarcely a day in which some of the old familiar series do not at some odd moment or other run through his head, usually quite automatically. He has never had any fondness for written computation for its own sake, and is perhaps, if anything, a trifle slower at it than the average man with an equal knowledge of mathematics. He is liable to occasional errors unless he carefully tests every stage of his work.[2]

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[1] Much the same thing was true of Safford; we are told (*Chambers's Journal*, VIII, p. 265) that it was his custom to talk to himself when originating new rules, but, by implication, not when carrying on computations by familiar rules.

[2] In the foregoing account an attempt has been made for the most part to avoid technical terms that would not be clear to the non-mathematical reader. The student of the theory of numbers will readily recognize that "2-figure endings" are least positive residues

III.

We are now ready to interpret the facts thus far set forth, and to construct from them an explanation of the mathematical prodigy.

Heredity. The table[1] gives such information as could be found concerning the heredity of the different calculators. Just what part these various circumstances actually played in the development of the different prodigies is a difficult question, which it would hardly be worth while to discuss here. This much is clear, however, that whatever the influence of heredity in some cases, it is in no sense an *explanation* of mental calculation, but at most a favoring circumstance. A satisfactory theory must rest on a much more definite basis than such general terms as heredity, environment, and the like can afford; it must explain the cases where hereditary influence is lacking, as well as those where such influence seems to be present. Hence we may safely leave the question of the relation of heredity to mental calculation for other investigators, and devote our attention to other questions.[2]

Development.—(*a*) *Precocity.* There is nothing more striking about the mathematical prodigies, nothing which has been the subject of more uncritical amazement, than their almost uniform precocity. Gauss began his calculations before he was 3 years old; the present writer, at 4; Ampère, between 3 and 5; Whately, at 5; Pugliese and Succaro, at about 5; Colburn, at 5: Safford, at 6 or earlier; Mathieu le Coq, Mr. Van R., of Utica, Bidder, Prolongeau, and Inaudi, at 6; Mondeux, at 7; the Countess of Mansfield's daughter, at 8 or earlier; Ferrol, Mangiamele, Grandmange, and Pierini, at early ages not definitely stated. Buxton's mental free beer record began from the age of 12; Zaneboni's calculations began at the same age; Dase attended school at the age of 2½, and took to the stage at 15. In short, precocity is unmistakably the rule; if we

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(modulus 100), and that the writer's process of changing odd numbers into even simply changes the modulus to 25 instead of 100, using residues which $\equiv 0 \pmod 4$). It would be easy to generalize many of the properties described above, and to show their application to n-figure endings and congruences in general; but such a task would carry us far beyond the limits of the present paper.

[1] See Appendix II.

[2] There are two points, however, on which a word may be said. In the first place, it is a pretty safe guess that Colburn's extra fingers and toes were an accident, as far as his calculating power was concerned, and had no connection with his mental abnormalities. On the other hand, the nervousness which he showed, and which he shared with Safford and an unnamed calculator in the neighborhood of Troy, N. Y. (*Memoir*, p. 173), may have predisposed him to less active participation in childish games, if not to actual illness, and so have increased the time available for his mental calculations.

count as unprecocious Zaneboni, Buxton, Dase, Diamandi (who began to calculate at 16, but had excelled in mathematics in school from the age of 7), the slave Tom Fuller, and the younger Bidder (about whom nothing definite is known in this respect), we have at the worst 6 unprecocious calculators as against 18 who were precocious.[1]

To understand this precocity we must note, first of all, that arithmetic is the most independent and self-sufficient of all the sciences. Given a knowledge of how to *count*, and later a few definitions, as in Bidder's case, and any child of average ability can go on, once his interest is accidentally aroused, and construct, unaided, practically the whole science of arithmetic, no matter how much or how little he knows of other things. Addition is only a shortened form of counting. The same is true of multiplication;[2] the writer's own case shows that the calculator need not even sit down and teach himself the multiplication table, as Bidder did, but may multiply by simple modifications of his counting process. Involution is simply a modification of multiplication; it has already been pointed out that the powers of numbers are natural resting-places in counting along the series of multiples of the numbers. The inverse operations of division and evolution grow naturally out of the direct operations of multiplication and involution; much more easily and naturally in mental than in written arithmetic. Once these elementary operations are mastered, such processes as reduction of years to seconds, compound interest, and any other arithmetical problems are simply a matter of understanding the meaning of the question and then applying known rules, plus a varying amount of ingenuity, to the solution. In accordance with the tendency of all mental operations, psychological shortenings of the processes involved will come with practice, and mathematical properties of the sort already described still further facilitate the work; so that in favorable cases the whole process may become in large measure automatic, and may go on while active attention is given to something else.

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[1] From this list has been omitted Huber's blind Swiss, who learned to calculate, presumably, late in life, by artificial methods, and obviously does not belong to what Binet calls the "natural family of great calculators." Binet's average of 8 years (*op. cit.*, p. 191) for the precocious calculators is too high; it is obtained by rejecting (without sufficient ground, so far as the writer can see) the cases of Gauss (3 years old) and Whately (who, as we have seen, began to calculate at 5, not at 3 as stated inadvertently in Scripture's table), and by taking in several cases the age at which the prodigy was *exhibited* before the Acad. des Sciences as the age when his calculations began. But on Binet's own showing, Mondeux had calculated for three years before he was exhibited in Paris; so that it will not do to average together such dissimilar data. Where the age of exhibition is later than 7, no attempt has been made to date the beginning of the calculations; if we then average the ages of the known cases of precocity (some of which are undoubtedly too high by a year or more), we get 5 to 5½ as an average, not 8. This is much more natural if the "natural calculator" usually begins to calculate from the time he learns to count. Of the six men not known to be precocious, two (Fuller and Buxton) were densely ignorant, and two of the others belong to the visual type, which, as we shall see later, is in certain respects intermediate between the "natural" or auditory and the "artificial" type.

[2] In multiplication the counting is, of course, done in the series of *multiples* of the multiplicand, not in the series of natural numbers; *cf.* part II of the present paper.

Moreover, the various symmetries and properties of numbers and series attract the attention of the calculator from the start, and keep up his interest until the habit of mental calculation has been firmly fixed. After that, if nothing intervenes to change that interest, there is practically no limit to which he may not attain, as the case of Dase abundantly shows.

We must note, furthermore, that practically an unlimited amount of time may be available for these calculations if the prodigy wishes so to use it. Mental arithmetic requires no instruments or apparatus, no audible practice that might disturb other members of the family, no information save such chance scraps as may be picked up almost anywhere for the asking, or absorbed, without even the trouble of asking questions, from older brothers and sisters as they discuss their school lessons. The young calculator can carry on his researches in bed, at the table,—if he allows himself to be "seen and not heard,"—during the perhaps laborious process of dressing or undressing; in short, at almost any time during the twelve or fourteen hours of his waking day, except when he is engaged in conversation or active physical play.

Thus, if an interest in counting once takes hold of a child either not fond of play or not physically able to indulge in it,—and stringing beads, counting the ticks of a clock, or even a chance question like "Let's hear if you can count up to 100", may start such an interest, which will then furnish all the material for its own development,—he may go on almost indefinitely, and become a prodigy long before his parents suspect the fact. Indeed, the interest in counting may seem so natural to the child that he may never think of doubting that every one else possesses it, and months or even years may elapse before some accident reveals the direction of his interest to his astonished relatives. Several of the calculators—Mondeux, Mangiamele, Pierini, Inaudi—were shepherd-boys, an occupation which, since it requires an ability to count and affords ample leisure, is peculiarly favorable for practicing calculation; several, again,—Grandmange (born without arms or legs), Safford, Pierini, the present writer,—were sick or otherwise incapacitated for active play to a greater or less extent, and thus enjoyed an equally good opportunity to practice calculation. Fuller and Buxton, on the other hand, whether precocious or not, were men of such limited intelligence that they could comprehend scarcely anything, either theoretical or practical, more complex than counting; and their purely manual occupations left their minds free to carry on almost without limit their slow and laborious calculations.

These considerations put the whole matter of mathematical precocity in a new light. Instead of joining in the popular admiration and awe of these youthful calculators,—and even psychologists have not been wholly free from this uncritical attitude,—we must say that precocity in calculation is one of the most natural things in the world. If a person is to become a calculator at all, he will usually begin as soon as he learns to count, and in most cases before he learns to read or write; and his development, while it will of course be gradual,—in Bidder's case probably a year elapsed between his learning to count and the early incidents which made his gift known,—will be so greatly facilitated by the amount of time available, the intrinsic interest of calculation, and the ease with which new information can be picked up as needed, that he may become a full-fledged calculator before he is suspected of being able to count without the aid of his fingers. His preoccupation with his calculations may give rise to a false appearance of backwardness, or he may really be of very low intelligence, or he may be an all-round prodigy like Safford, Gauss, and Ampère; mental arithmetic is so completely independent and self-sufficient that it is equally compatible with average endowments or with either extreme of intelligence or stupidity.

Mathematical precocity, then, stands in a class by itself, as a natural result of the simplicity and isolation of mental arithmetic. There is nothing wonderful or incredible about it. The all-round prodigy like Ampère or Sir William Rowan Hamilton or Macaulay is possible only in a well-to-do and cultured family, where books are at hand and general conditions are favorable, and he must possess genuine mental ability. The musical prodigy, again,—Mozart is the stock instance,—must come of a musical family, hear music, and have at least some chance to practice, and hence cannot long hide his light under a bushel. But the mathematical prodigy requires neither the mental ability and cultured surroundings of the one nor the external aids of the other. He may be an all-round prodigy as well, like Gauss, Ampère, and Safford; it is not improbable that Bidder, under favorable conditions, would have developed into such an "infant phenomenon"; but he may also come of the humblest family, and be unable, even under the most favorable conditions, to develop average intelligence. He may proclaim himself to the world almost at once, like the all-round or the musical prodigy, or keep his gift a secret for months or even years. If we are to call him a prodigy at all, it is important to realize how widely he may differ from other prodigies, and to avoid carefully the popular confusion due to the misleading associations of the words "prodigy" and "precocious."

(*b*) *Loss of Power*. Mental calculation, then, starts from an interest in counting; at the outset it demands only that ability to count by 1's, 2's, 3's, 7's, and the like, which all of us require for such every-day purposes as keeping track of the days of the week. But if for any reason this interest in counting is lost, practice in calculation will cease, and the skill already acquired will disappear, just as the pianist's skill is lost when interest and practice cease. There are two striking instances of this among mental calculators: Whately and Mr. Van R. of Utica, both of whom began to calculate at an early age, but lost the power after two or three years. Here, again, however, there need be no mystery; the disappearance of the gift with the loss of the interest in which it originated is as natural and normal as its original appearance.

Just what caused the loss of interest is not always easy to say. In Whately's case the trouble may have been that on going to school he was taught arithmetic or "ciphering" by methods very different from his old ones, became confused, failed to establish a connection between the two, and lost his interest in calculation as a result of his distaste for "ciphering." In Colburn's case the loss of skill seems to have been much more gradual, and probably never complete. In this respect he is like the pianist who retains his interest in music, but is prevented by other occupations from keeping in practice; if later on he is able to resume practicing, his skill is soon regained.

Education. A glance at the table of mathematical prodigies[1] will show that education as such, whether mathematical or general, has little or no influence on the calculating power, either to help or to hinder it. At the one extreme we find Fuller and Buxton, men of dense ignorance and limited powers of calculation, and near them Dase, the greatest of all calculators, who even in mathematics was scarcely less stupid. At the other extreme stand Ampère, Gauss, Bidder, and Safford, in whom unusual mathematical and general ability and a wide range of interests exist side by side with marked skill in mental calculation; while, on the other hand, the ordinary mathematician or man of culture has little or no gift for mental arithmetic. That the calculating power should be independent of general education is not particularly surprising; but its independence of mathematical training and ability seems at first less natural and obvious.

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[1] See Appendix II.

In a general way, we may distinguish three grades of mathematical ability in the great calculators. Those of the first class never get beyond the stage of pure counting, though of course the counting process comes to be abbreviated more or less with practice. At this stage the point of view is not even arithmetical; the calculator thinks not of arithmetical operations, but of *properties* of numbers and of series, and the short-cuts he uses are of a relatively simple sort, showing no mathematical insight. Without insisting too sharply on the distinction, we may term these men "calculating prodigies."

Those of the second class may be called, from the present point of view, "arithmetical prodigies"; Colburn and Dase will serve as examples. Here we find a fairly well developed knowledge of arithmetic, and a distinctly arithmetical point of view; it is operations of calculation, rather than mere properties of numbers, in which these men are interested, and the various short-cuts used are, we may suppose, suggested by practice in calculation rather than by mathematical keenness.

The third class comprises the "mathematical prodigies"[1] proper, of whom Bidder may be taken as the type. Here we find real mathematical ability, power to take a distinctly algebraic point of view, to generalize, and hence to discover all sorts of ingenious short-cuts and symmetries. Bidder's compound interest method is perhaps the most striking example; Mondeux's unconscious use of the binomial theorem is another.

Such a classification must not be taken too seriously, of course; a good deal of hair-splitting would certainly be needed to establish hard and fast lines between the different classes. The important point is that mental calculation and mathematical ability are essentially independent, and that almost any degree of the latter is compatible with any degree of skill in the former. Where the two are found together, calculation usually appears first; but even to this there are exceptions, since Diamandi excelled in mathematics at school for nine years before he discovered his gift for calculation.

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[1] These terms are here used merely as a convenient means of temporarily designating different grades of skill in calculation; the writer would not advocate their general adoption as here defined. For a general term embracing all three classes, "arithmetical prodigies" seems the best; the reason this term was not taken as the title of the present paper was that Scripture's article already bore that title, and it seemed undesirable to run the risk of confusing later students by adopting it for a second article on the subject.

Neither mathematical nor general education or mental ability, then, has any *direct* influence on mental calculation. Indirectly, however, education may have an important influence. We have seen that if for any reason the interest in calculation is lost, the calculating power will disappear. Now mental calculation is a narrow and special field, with little practical importance for most men; hence, other things being equal, as a boy's sphere of interests widens, his interest in mental calculation is likely to sink into the background. This explains why so many ignorant men have excelled as calculators; ignorance, by preventing the intrusion of other interests, leaves the calculator free to develop his one gift, and keeps him from realizing how trivial it is, and how groundless is the public amazement which, perhaps, contributes to his support. On the other hand, if the interest in calculation is retained despite the widening of the sphere of interests resulting from education, the calculating power may prove to be of considerable practical value. The two Bidders will serve as examples. The father owed his striking success as an engineer primarily to his powers of mental calculation, which not only won him the friends who contributed to pay the cost of his education, but were of constant use to him in his profession, especially as an expert witness before Parliamentary committees. The son, a lawyer, tells us that he finds it an immense advantage to have in mind a number of formulas and constants for ready reference,[1] and doubtless his readiness in using these formulas and constants in mental arithmetic was still more useful. Gauss and Safford are illustrations of the obvious possible usefulness of mental calculation to the mathematician.

Calculation. If mental calculation naturally arises out of counting, we might at first suppose that addition would be the favorite operation of the mathematical prodigies; but there is no evidence to this effect in any known case. Bidder specifically states that multiplication is the fundamental operation; Colburn found multiplication easier even than addition or subtraction; Buxton's favorite problems seem to have been long multiplications, yet we have seen reason to suspect that his calculations never progressed far beyond the counting stage; the younger Bidder performed 15-figure multiplications, Safford (though in a very easy case) an 18-figure multiplication, and Dase one of 100 figures which did not seem seriously to tax his powers. The reduction of years to seconds and similar problems, resting on simple applications of multiplication, have been favorites with many calculators. In short, *multiplication* and not addition seems to be the fundamental and favorite operation in mental calculation.

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[1] *Spectator*, LI, 1878, p. 1635.

Nor is this difficult to understand. It has been suggested above that in the earlier stages the "natural" calculator who begins with counting—as distinguished from the "artificial" calculator who begins relatively late in life, using book-methods from the start—is interested mainly in *properties* of numbers and of simple series. Now these properties are revealed not by addition, but by multiplication, or the forms of counting which are equivalent to multiplication. Addition and subtraction bring out no properties of particular interest. Given any number, another number can be added to it or subtracted from it so as to produce any other number whatever, by processes which are mechanical and not particularly interesting. Where the addition of a series of terms does produce a result of any interest, as in the series $1+2+3+4\ .\ .\ .\ .$, or the series $1+3+5+7\ .\ .\ .\ .$, the interesting property belongs to the sum not primarily as a sum, but as a function or *multiple* of the nth term: in the first case the sum of n terms is $\frac{1}{2}n(n+1)$, which is a multiple of the next term $n+1$, or of half the next term in case that next term is even; while in the second case the sum of n terms is always equal to n^2, which is the *product* of two equal factors. Addition and subtraction, in short, apart from multiplication, are mechanical processes, and are of very limited interest to the calculator, whereas multiplication is the key to all those properties which arouse his interest and stimulate him to establish the calculating habit. The differences between odd and even numbers, the properties of prime and composite numbers, as well as of squares, cubes, and other powers, series of all sorts, 2-figure endings, casting out the 9's, and the like, all grow directly out of multiplication.

The *methods* used in mental multiplication are various. The simplest is, as we have seen, a more or less abbreviated process of *counting*. It is of course difficult to say just which individual calculators remained permanently in this stage; we have seen, however, how large a part this counting process plays in the writer's own case, and it is not unlikely that this was the method of both Fuller and Buxton.

Several of the prodigies began multiplying at the *left*, instead of at the right as in the ordinary written method. The advantage of this procedure, as Colburn and Bidder explain, is that the larger figures first obtained are easily remembered, because ending with so many zeros, and that there is at any given stage of the work only one result to keep in mind. The result is changed, to be sure, by each new partial product incorporated with it; but the great difference between written and mental arithmetic is that whereas in the former it is easiest to record the partial results and later combine them all at once, in the latter it is much easier to combine at each separate stage, and relieve the memory of the strain of remembering the partial results throughout the process. Probably in most cases the partial products actually *are* remembered, as by Inaudi, *e. g.*; but the fact that they *may* be forgotten if desired without interfering with the calculation, relieves the calculator of all anxiety in the matter.

Such is, in general, Bidder's explanation of this method. It is obvious, however, that no such carefully reasoned considerations can influence the calculator at the early stage when his methods are taking shape; and in mental arithmetic it is almost always easier to go on with an old method, however imperfect in theory, than to learn a new one. The real reason for Bidder's adoption of this method is doubtless the very simple one that it is more natural to begin with the first figure of the multiplier than with the last; if, however, the calculator should accidentally form the habit of beginning with the last figure, it is hard to see where any real inconvenience would result. In mental as in written arithmetic, much depends on custom and habit; it is hard to see any great difference in convenience between beginning at the right and beginning at the left, either in mental or in written multiplication. Of the two processes below,

$$\begin{array}{r} 256 \\ 243 \\ \hline 768 \\ 1024 \\ 512 \\ \hline 62208 \end{array} \qquad\qquad \begin{array}{r} 256 \\ 243 \\ \hline 512 \\ 1024 \\ 768 \\ \hline 62208 \end{array}$$

it is custom, rather than the minute difference in convenience, that sanctions the first rather than the second.

There are two calculators, the younger Bidder and Diamandi, who are known to have used *cross-multiplication;* the elder Bidder, however, as we have seen, did not use this method, despite his son's statement to that effect. It is probably not a coincidence that both the users of cross-multiplication were of the visual type; for while this method has advantages which have already been explained, it is a radical departure from direct counting, and would naturally arise only as an abbreviation of *written* multiplication, or of mental multiplication done by the methods of written multiplication. In short, it is an "artificial" method of mental calculation. Cross-multiplication would be greatly facilitated by a mental picture of the figures as arranged for written work; and we know that both these men possessed such mental pictures, though doubtless in a modified form. Diamandi learned mental arithmetic after his methods of written arithmetic had taken definite shape, and it is fairly safe to assume that the same was also true of the younger Bidder; this would imply, of course, that he was less precocious than his father.

Another method of multiplication possible for a visual calculator is that explained by Richard A. Proctor.[1] In brief, according to this explanation, each number is for the calculator a visual group or *pattern of dots*, discs, or the like, and multiplication consists in mentally juxtaposing or otherwise combining as many of these patterns of the multiplicand as there are units in the multiplier, and then rearranging the dots into a simpler pattern which indicates the product at a glance. Of course all sorts of short-cuts would come in with practice; but in principle the method is as indicated. The essential characteristic of these mental dot-patterns is their plasticity, the ease with which the pattern can be changed while the number of dots remains unaltered. Proctor tells us that as a child he himself acquired some little facility in mental arithmetic by this method, and probably with sufficient interest and practice it would give good results in the case of a visual calculator. Up to the present time, however, Proctor's case is the only one of the sort that has come to light, and there is no ground for extending this explanation to other known cases.

Another explanation of mental multiplication, proposed by Proctor and Scripture, attributes to the prodigies an *extended multiplication table*. Proctor conjectures that it may even reach (in the case of Bidder) to 1,000 x 1,000; Scripture, however, suggests only 100 x 100. On this theory, the multiplication of two 12-figure numbers would proceed by the division of each number into six 2-figure periods, or four 3-figure periods, which would then be used in the same way as ordinary mortals use single figures, giving the answer in 6 partial products of 6 operations each, or 4 partial products of 4 operations each, instead of 12 partial products of 12 operations each.

Doubtless such a method is theoretically possible, and would materially reduce the time required for multiplication; but can we attribute it to any calculator of whom we have specific knowledge? We know, of course, that any book-method or artificial aid of written or mental arithmetic can be utilized with sufficient practice. Teachers have taught whole classes of pupils to multiply with the aid of more or less extended tables; Gauss used logarithms in his mental calculations; and the younger Bidder, Ferrol, and probably Huber's blind Swiss used mnemonics of some sort. The question is, however: Did such men as Buxton, Colburn, Mondeux, Dase, and Bidder—men who undoubtedly belong to the "natural family of great calculators," *i. e.*, whose methods of calculation took shape in the first instance independently of books—use multiplication tables reaching to 100 x 100, or even 50 x 50, or can their feats be explained without presupposing a table reaching beyond 10 x 10 or 12 x 12?

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[1] *Cornhill Mag.*, XXXII, 1875, p. 157; *Belgravia*, XXXVIII, 1879, p. 450.

There are two ways in which such an extended multiplication table might be acquired. In the first place, the calculator might sit down and figure out the various products, either mentally or on paper, and then commit them to memory, a few at a time, until all were fully mastered. In this case the multiplication of 48 by 64 would take the mental form, "Forty-eight times sixty-four are 3,072," or, in a shortened form, the numbers 48 and 64, connected by the multiplication sign or the words "multiplied by," would call up directly the idea of 3,072, without any intermediate calculation, either clearly conscious or automatic; just as 9 x 7 means 63 for the ordinary man, without the intervention of any of the other multiples of 9 or 7. Fatigue or lack of practice might render the process slower, but would not introduce any intermediate links of calculation. In the second place, the calculator might, with practice, be able to multiply 48 x 64 so readily and rapidly, by more or less automatic processes, that he would get the answer, 3,072, as quickly as if he had relied on a direct act of memory; and if the process of calculation happened to be mainly or wholly automatic, he might even be ignorant of its existence, and suppose he had actually found the result by direct and unaided memory. At times, however, brief flashes of these intermediate calculations would pass through his mind; and when he was tired or out of practice, his calculations would not only be slower, but would be of a more clearly conscious character. The intermediate links, which had been made automatic and half unconscious by practice, would return as soon as fatigue or disuse reduced the calculator's speed.

In this second case, however, it is evident that the whole 5,000 or 10,000 entries of a multiplication table to 100 x 100 cannot properly be said to exist already computed in the calculator's mind. We shall have to discuss a little later the various ways of shortening the calculation on the psychological side; but in dealing with the enlarged multiplication table theory, we must insist that the only legitimate interpretation of the theory is that such a table is *deliberately committed to memory* by the calculator, and not reached in particular cases by an abbreviated process of calculation whose omitted links could return under any ordinary circumstances. Fatigue or lack of practice might prolong the time of unmeditated recall, but could never interpolate into it even the briefest flashes of calculation. If, then, no specific evidence is at hand that such an enlarged table actually *was* used by any of the "natural calculators,"—and no such evidence has been adduced by the advocates of the theory,—we cannot accept this explanation unless it is shown, either that the enlarged multiplication table is a more natural method than the smaller table, or that the actual achievements of some or all of the calculators cannot be explained in any other way. Actually, the latter of these arguments is the one on which the defenders of the theory seem to rely; it will be safest, however, for us to examine the former as well.

In the first place, then, it can hardly be claimed that a multiplication table extending much beyond 10 x 10 is either natural or useful in the early stages of mental calculation, if the calculation arises in the natural and not in the artificial way. The child who becomes a calculator begins to multiply soon after he learns to count, certainly before he has learned to count beyond 1000, and hence before his multipliers have exceeded 31. It will hardly be argued that up to this point a table beyond 10 x 10 would be of any use. Hence *the calculator's habits and methods of multiplication are definitely formed before a table beyond 10 x 10 is needed.* Such a table serves all the purposes even of written multiplication for most of us; in fact, even if we know the multiplication table to 12 x 12, we rarely use it beyond 10 x 10. How many of us in multiplying 412,976 by 3,128, for instance, would think to treat the 12 in either the multiplicand or the multiplier as a single factor? It is only where the multiplier itself is simply 11 or 12 that the 12 x 12 table excels the 10 x 10 table for any practical purpose. Hence, unless an enlarged table extended all the way to 100 x 100, its utility would be relatively slight. In mental multiplication, too, we know that Inaudi, who could multiply 6 figures by 6 figures, used a table reaching only to 10 x 10. We are thus brought to our second question: In multiplication by 12-figure numbers or larger, need we presuppose a table extending beyond 10 x 10 in order to explain the actual achievements of known calculators? Must the calculator enlarge his table as the size of the numbers he uses is increased, or does he simply depend on practice, and on new short-cuts other than enlargement of his multiplication table, to extend his powers and increase his speed?

Bidder's answer to this question in his own case is explicit.

He says:[1] "Now, for instance, suppose that I had to multiply 89 by 73, I should instantly say 6,497; if I read the figures written out before me I could not express a result more correctly, or more rapidly; this facility has, however, tended to deceive me, for I fancied that I possessed a multiplication table up to 100 x 100, and, when in full practice, even beyond that; but I was in error; the fact is that I go through the entire operation of the computation in that short interval of time which it takes me to announce the result to you. I multiply 80 by 70, 80 by 3; 9 by 70, and 9 by 3; which will be the whole of the process as expressed algebraically, and then I add them up in what appears to be merely an instant of time."

This testimony is unequivocal; Bidder, as Scripture admits, certainly did not have a multiplication table extending beyond 10 x 10, yet he was able, on one occasion at any rate, to perform a 12-figure multiplication. Furthermore, Bidder's calculations were so rapid and automatic that he was himself deceived in this matter, and thought he actually had such a multiplication table as Proctor attributes to him. Even if we grant that when Bidder wrote this account, in his 50th year, his powers had slightly diminished, with the result that his calculations were somewhat less automatic than in his youth, it is clear, nevertheless, that even in his prime as a calculator he depended on calculation, not on a memorized multiplication table beyond 10 x 10.

What shall we say, now, of other calculators? Fuller, the man who required 2 minutes for a simple calendar problem, may be dismissed at once; he needed no enlarged multiplication table. Buxton, Scripture tells us,[2] "preserved the several processes of multiplying the multiplicand by each figure of the lower line in their relative order, and place as on paper until the final product was found." The reference for this passage is not given. The statement, however, is directly contradicted by Buxton's own account of his method of multiplication, according to which, as we have seen, he multiplied by 5, then by 20, then by 3 to multiply by 300.[3] It is safe to omit Buxton, therefore, from the list of users of large multiplication tables. Colburn's account of his method of multiplication[4] is almost identical with Bidder's, and leaves no doubt that he multiplied by but one figure at a time. Mondeux, we are told,[1] "actually possessed part of such a table." Here again, unfortunately, the reference is omitted. The only statement in Cauchy's report that looks like a foundation for this assertion is that Mondeux "knows almost by heart the squares of all whole numbers under 100." But, as we have already seen, the powers of numbers strongly attract the interest of the calculator at an early stage; hence in the absence of other evidence it is not legitimate to infer the presence of a complete multiplication table, or any essential part of it, from the presence of a table of squares. Inaudi's methods are fully described by Binet, and show that he used a table extending only to 10 x 10. Diamandi and the younger Bidder used cross-multiplication, and there is no definite evidence that they used more than one figure at a time. Safford so closely resembles Bidder and Colburn that, in the absence of evidence to the contrary, we may safely assume that his method of multiplication was the same as theirs. Zaneboni did not possess a systematic table extending to 100 x 100, though he knew many squares and a few scattered products beyond 10 x 10.[2] Concerning Ampère so little is definitely known that it is idle to speculate about his methods; but this is hardly a proof that he used an enlarged multiplication table.

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[1] *Proc. Inst. C. E.*, XV, p. 256.

[2] *Op. cit.*, p. 58. On this statement, which is almost certainly incorrect, Scripture bases his belief that Buxton had a good "imagination," *i. e.*, belonged to the visual memory type.

[3] In the light of this example, which he himself quotes (p. 48), it is hard to understand *why* Scripture thinks (p. 46) we can presuppose the enlarged multiplication table in the case of Buxton.

[4] *Memoir*, pp. 189-191.

There remain, then, only Gauss and Dase. It must be admitted that these men *may* have used enlarged multiplication tables; the assertion cannot be disproved on the basis of available evidence, and both men seem to have excelled practically all other calculators. But while Gauss undoubtedly began as a "natural" calculator, he afterwards also used logarithms and other "artificial" methods in his mental calculations; so that, even if we grant—though there is no known evidence for the view—that he used a large memorized multiplication table, his case affords no basis for inferences concerning the procedure of other calculators less gifted mathematically. Thus the case of Dase alone among the "major calculators" remains for further consideration.

We have already seen that in rapidity and extent of calculation Dase stands in a class apart. It would therefore be unsafe to say dogmatically that his methods did not differ from those of other calculators. Even in his case, however, the ordinary methods are sufficient to explain all his feats, without recourse to the extended multiplication table theory. Inaudi could perform a 2-figure multiplication, by the ordinary method, in 2 seconds, the partial products involving 6 figures, or at the rate of 180 figures of partial product per minute. Dase required 54 seconds for an 8-figure multiplication, with not more than 72 figures of partial product, or 80 figures per minute, a falling off of about 55% from Inaudi's speed in the shorter operation. For any other calculator a much greater falling off would naturally be expected; but in a 40-figure multiplication, performed in 40 minutes, Dase was able to maintain a speed of 41 figures of partial product per minute, and in a 100-figure multiplication a speed of about 20 figures per minute, in complete defiance of the rules that hold for other calculators.[1] Hence even if we grant that Dase divided the numbers into 3-figure periods, he may well have multiplied within those periods by the ordinary method, without having recourse to a table larger than 10 x 10.

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[1] Scripture, *op. cit.*, p. 46.
[2] *Riv. sper. di Fren.*, XXIII, 1897, p. 411.

There is no warrant, then, for supposing that any of the prodigies except Gauss and Dase used a multiplication table larger than that of ordinary mortals; and even in these two cases there is no direct evidence, only a bare possibility; all their known feats are explicable on the supposition that they used the small tables of Bidder, Inaudi, and the rest. We may therefore dismiss the theory of enlarged multiplication tables, at least until its advocates have brought forward further and more definite evidence than any that has yet been produced.

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[1] In Binet's tests Inaudi performed a 2-figure multiplication in 2 seconds, 3-figure in 6.4 seconds, 4-figure (the limit of his ordinary stage exhibitions) in 21 seconds, 5-figure in 40 seconds, but 6-figure in 4 minutes, or 240 seconds. This sudden increase in the time, 6 times as long for 6 figures as for 5 figures, seems to indicate that in passing from 5 to 6 figures Inaudi exceeded the limits of his ordinary practice, and became confused. Bidder (*Proc. Inst. C. E.*, XV, p. 256) suggests that in cases where the multiplicand contains the same number of figures as the multiplier, the difficulty should increase roughly as the fourth power of the number of figures in the multiplier, since the number of partial product figures involved increases as the square of the number of figures in the multiplier, while the difficulty of remembering the larger numbers also increases roughly as the square of the number of figures in the multiplier. On this basis, the time for 6 figures ought to be about twice as long as for 5 figures, whereas with Inaudi it was 6 times as long, or 3 times the theoretical speed. In Dase's case, however, the results are all in the other direction. In passing from 8 figures to 40 figures his time increased only about 44-fold, or 1-14 the theoretical increase (625 times); and in passing from 40 figures to 100 figures the increase in time was only 13 instead of 39 times, or ⅓ what we should expect on Bidder's theory. Of course these comparisons cannot pretend to be exact; still they are interesting, as showing how little Dase's powers were strained by increase in the size of the numbers. Nowhere in his calculations is there any indication of such confusion as overcame Inaudi in passing from 5 figures to 6 figures. In fact, we may well suppose that only physical fatigue could limit the extent of Dase's calculations in a single sitting. If he could, like Buxton, after a night's rest resume his work where he left off on the previous day, it is safe to assume that even a 200-figure multiplication would hardly have been beyond his powers.

Factoring was a favorite problem with Colburn, Bidder, and Safford; none of the others, however, are known to have explored this field. Colburn and Bidder, and doubtless Safford[1] as well, used 2-figure endings in solving problems of this class.

We have seen that *square and cube root* problems were favorites with several of the prodigies. Here, as in multiplication, Dase towers above all the rest; he could extract the 30-figure square root of a 60-figure number in an "incredibly short time," and the 50-figure square root of a 100-figure number in 52 minutes. His method was probably similar to the ordinary written method, but with the short-cuts suggested by his great familiarity with large numbers. Whether he found part of the numbers by simple division, or preferred to continue his approximations in the ordinary way to the end of the process, is not known. He was almost certainly acquainted with some of the properties of 2-figure endings,[2] and may have used them in finding the roots of small numbers. More probably, however, he knew by heart the squares of all numbers at least up to 100, in which case 2-figure endings would be of less use to him. In dealing with numbers of 60 or 100 figures they would be practically useless, for the reason that where the given number is as long as this, it is usually selected by the questioner at random, rather than formed by laboriously squaring a large number. Hence the given number will rarely be a perfect square, and the method of endings will not give even the last two or three figures. Whether it can be used at any of the intermediate stages of the work is doubtful.[1]

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[1] The evidence in Safford's case is as follows: (1) The general similarity between Safford on the one hand, and Colburn and Bidder on the other. All three specialized in evolution and factoring, where 2-figure endings are peculiarly helpful; Colburn and Bidder tell us explicitly that they used these endings; hence it is probable that Safford did also. (2) Safford extracted the cube roots of three 7-figure numbers "instantly." Now the ending method is instantaneous for numbers up to 9 figures, and is the only known method that is instantaneous. (3) When asked the cube root of 3,723,875, he answered, "155, is it not?" whereas in the case of numbers not ending with 5 his answers were categorical. Now for most odd endings there is only one possible cube root; but for the ending 75 there is a choice between 15, 35, 55, 75, and 95. A slight hesitation between 135 and 155, or 155 and 175, would therefore be natural if he depended on the ending primarily, whereas if he depended on memorization of other cubes, or direct calculation, multiples of 5 would be no harder than other numbers.

[2] The evidence in this case is that Dase was fond of finding 5th powers, because the last figure was always the same in the 5th power as in the given number. (Gauss-Schumacher *Briefwechsel*, V, p. 382.) Now the 5th power also has the property that, in many cases, the last figure alone of the given number determines the last *two* figures of the 5th power. Any one who had found 5th powers often enough to acquire a preference for them could hardly fail to note this property, which in turn might easily lead to the discovery of the properties of 10th and 20th powers, especially since 10th and 20th powers are also themselves 5th powers. But since at the 20th power there are only four possible endings,—00 for numbers ending with 0, 25 for numbers ending with 5, 01 for all other odd numbers, and 76 for all other even numbers,—and since after the 20th power the cycle of endings repeats,—the 21st power ending being usually the same as the 1st, the 22nd always the same as the 2nd, etc.,—the properties of the whole cycle are readily investigated, and would be almost certain to attract the calculator's attention, as soon as he had become really interested in 5th powers.

Most of the other prodigies who have made a specialty of square and cube root problems either depended on 2-figure endings, resorting to guess and trial when the given numbers were not perfect squares or cubes, or else kept in mind the squares and cubes of many or most of the numbers up to 100 or beyond. Such methods would work fairly well when the root contained not more than 5 or 6 figures. Answers can be obtained "instantly," however, only by the method of endings, and even by that method only when there are not more than 3 figures in the root; though the method might be made "instantaneous" for 4-figure roots of exact squares and cubes if the calculator had committed to memory the squares and cubes of all numbers under 100.

Inasmuch as the part played by 2-figure endings has hitherto been little recognized by students of the mathematical prodigies, it seems worth while here to determine, if possible, just how widely they were used by the different calculators. Colburn, Bidder, Safford, Zaneboni, and the present writer, we have seen, used them more or less freely. There is some evidence, not conclusive, however, that Dase was familiar with their properties. The daughter of the Countess of Mansfield extracted readily the square and cube roots of 9-figure numbers, and may have used this method. Gauss was familiar with many propositions in number-theory, of which the properties of 2-figure endings are only special cases; it would hardly be safe to infer, however, that he used these properties extensively in his mental calculations. A bare possibility exists in the case of one or two other calculators—Mondeux, for instance—that the properties of the endings were more or less fully known. Without seeking to exaggerate the importance of these endings, therefore, we may safely say that of the calculators we have studied, about one in every four was familiar with them. Their importance for us here, however, is rather as an illustration of the sort of numerical symmetries and properties which arouse the interest of the calculator than as new and valuable discoveries; in fact, from the mathematical standpoint they are trivial and of very limited interest. To answer questions in evolution and factoring, the mathematician would turn to his tables of factors or roots, or to a logarithm table; he would regard the properties of the mathematical prodigy's 2-figure endings as unimportant special cases of more general propositions in the theory of numbers. Up to the present time, then, these endings are of merely curious interest except in connection with mental calculation; though it is conceivable, of course, that if a new and comprehensive theory of their properties were worked out, it might find a subordinate place in the theory of numbers.[1]

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[1] John Wallis (1616-1703), according to letters of his reprinted in the *Classical Journal*, XI, 1815, p. 179, and in the *Spectator*, LII, 1879, p. 11, extracted the square root of 3,00000,00000,00000,00000,00000,00000,00000,00000 mentally, and on another occasion the square root of a 53-figure number. The third figure of the first answer is wrongly given in both journals as 7 instead of 3; as the next 3 or 4 figures are correct, however, for $\sqrt{3}$, the error is probably only typographical. In the *Spectator* the first problem is wrongly given as 30000, etc., instead of 3,00000, etc. Scripture (*op. cit.*, p. 38) carries over both errors from the *Spectator* without comment.

It will be noted that the term "ending" has been used in the present paper as a synonym for "2-figure ending," the implication being that no other kinds of endings are of importance in mental calculation. It is easy to see why this should be so. The properties of 1-figure endings, or last figures, are so simple that many of them are familiar to the average schoolboy, and they tell so little about the answer to a given problem that their utility in mental arithmetic is negligible. To study 3-figure endings, on the other hand, would involve a large number of 3-figure multiplications; so that here, as in the case of the multiplication table, if the endings are used at all, the use of the smaller (2-figure) endings becomes a fixed habit long before larger ones could be of any real use. The only known exception occurs where the given power ends with 5; in this case there is an unusually large number of possible roots, and in seeking for some way of choosing between them, some of the calculators (Bidder, for example) noted what 2-figure roots corresponded to the various 3-figure endings for multiples of 5. It is obvious, however, that the use of 3-figure endings in this exceptional case would not warrant the inference that they were used in any other case; and Bidder's testimony shows clearly that he, at any rate, made no further use of them.

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[1] In one sense, however, these endings may be regarded as the properties or functions of numbers analogous to logarithms, and previously unknown to mathematicians, which some writers suggested might explain Colburn's rapidity in calculation, especially in evolution and factoring. (*Phil. Mag.*, XL, 1812, p. 125; *Analectic Mag.*, I, 1813, p. 128.)

Many simple *algebraic problems* were solved mentally by the different calculators. Those who had a general mathematical training of some sort—Ferrol, Safford, and Gauss, for example—doubtless solved these problems by algebraic methods. Many of the others, however, depended on simple trial, or on little tricks which are readily discovered for certain classes of problems, and need not be dwelt on here. It is safest to assume that the method was one of simple trial in all cases where there is not definite evidence to the contrary.

Bidder's method of solving *compound interest* problems has already been described. In this particular branch he seems never to have had a rival.

Arithmetical Association. Under this caption Scripture,[1] in part following De Morgan and others, has given an account of some of the ways in which the psychological processes of mental calculation can be shortened. He points out, in particular, that with practice the mental calculator can easily train himself to omit useless words, and think only of the *numbers* concerned; thus instead of saying, "3 and 4 are 7," "3 times 7 are 21, put down 1 and carry 2," and the like, he can learn to say simply, "3, 4, 7," "3, 7, 21, 1, 2," etc. As De Morgan expresses it,[2] "Don't *say* 'carry 3,' but do it." It is evident, furthermore, that as the process gradually becomes more and more familiar and automatic, many of the intermediate steps of the computation may partly sink into the background of consciousness, perhaps even disappearing altogether from the field of attention; thus, as we have already seen, the intermediate links in Bidder's 2-figure multiplications were at one time so completely automatic that he believed they were altogether absent, and supposed he possessed a multiplication table extending to 100 x 100 or even beyond. Moreover, where the given numbers are familiar, only part of a number may be clearly conscious, even when the whole number functions in the calculation. Thus in the present writer's case the ending of a number may for many purposes completely replace the number itself. A further abbreviation which may be noted is that, in the case of an auditory calculator, any chance visual associations which may be present in the early stages of his calculations, but which play no active part in them, may gradually drop out as his skill increases. The same thing may happen, in certain cases, with the associated motor tendencies, if they are not too strong. These psychological short-cuts, in connection with the mathematical short-cuts to which we have already referred, explain the great rapidity attained by some of the prodigies in their mental operations.

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[1] *Op. cit.*, p. 42 ff.

[2] *Elements of Arithmetic.* London, 1856, p. 164. In this work (pp. 161-5) will be found a very clear account of the natural counting process, with some of its modifications and short-cuts, by which the "natural" mental calculator is developed.

A certain amount of caution is necessary, however, in attributing these various short-cuts to individual calculators, except where we have their own explicit testimony as a check. Binet was able to establish that Inaudi, in his mental multiplications, made little, if any, use of these short-cuts; and simple repetition of the unabbreviated calculations will produce a considerable increase of speed without further aid in the way of mathematical or even psychological short-cuts of the sort just described. We have seen how little ground there is for attributing extensive multiplication tables to most of the prodigies; yet some writers have supposed that this was the simplest, if not the only, explanation of the speed with which many of the prodigies could calculate. In theory, there is no definite limit to the short-cuts possible in mental calculation, and it is evident that in the case of such a man as Dase a good many of these short-cuts must be actually realized in practice; at the same time, it is not safe to attribute any *particular* short-cut to a given calculator unless specific evidence is at hand that he actually used that particular method and not some other equally rapid.

Memory. (*a*) *Memory versus Calculation.* It has become customary, in the literature on mathematical prodigies, to distinguish more or less sharply the parts played by memory and by calculation proper in the various operations, especially where the numbers dealt with are very long. Bidder tells us that only the limits of his memory would stand in the way of performing immense calculations in an incredibly short time. Buxton was excessively slow in calculating, but had such a tenacious memory that he could work on a problem for weeks, and so solve almost any problem, long or short, that happened to arouse his interest. Dase, according to this view, differed from ordinary mortals mainly in the possession of a wonderful memory; but as far as the calculation proper is concerned, we have the authority of Gauss for the statement that Dase's 100-figure mental multiplication, which required 8¾ hours, could be done on paper in perhaps half the time.[1] This view implies, then, that the processes of the calculator in a very long mental multiplication are essentially the same as in a very short one, being in either case substantially those of written multiplication; in other words that the work, computed bit by bit in the long as in the short multiplication, is retained in the memory in a more or less isolated and mechanical fashion, just as on paper the partial products are first computed and recorded, figure by figure, and then added. Practically, then, the view is a sort of common-sense faculty psychology, seeking to justify itself by pointing to the difference in speed between mental and written multiplication.

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[1] Gauss-Schumacher *Briefwechsel*, V, p. 297.

Now it can hardly be questioned that the mathematical prodigy's figure-memory is superior to the ordinary man's. Dealing constantly with figures, the mental calculator learns to assimilate them readily. A 20-figure number, which for most of us is a meaningless string of figures devoid of interest, for him "makes sense," and so is easy to learn, just as a page of French is more easily learned by a Frenchman than by a foreigner who knows little or nothing of the language. Hence we may safely assume that the calculator's figure-memory will outstrip his powers of calculation; that is to say, by the time he can mentally multiply two 6-figure numbers, let us say, he will have little or no trouble in remembering almost at a glance, or after a single hearing, two numbers of 7 or 8 figures each. This is on the supposition, of course, that he has never practiced remembering long numbers merely for their own sake; we must here distinguish those calculators, like Zaneboni, who were fond of committing statistics or very long numbers to memory without any immediate intention of using these numbers in mental calculations, from those who made no effort to develop their memory for figures except in the service of calculation.

The question is, then: Will special practice in memorizing numbers, apart from their use in calculation, extend the powers of the prodigy in calculation proper? In other words, does he use the same mathematical and psychological processes in a long multiplication as in a short one, except that in the former he stops every now and then to deposit some of his partial results temporarily in his memory, later returning and picking them up in order to unite them into a final result? In written arithmetic we compute a series of partial results, register these results on paper at each stage, and later come back to unite them, by a separate operation, into the total product; is this the general scheme of mental multiplication also?

It would perhaps be unsafe to answer this question dogmatically in the negative; at the same time, we should have to distinguish as an "artificial" calculator any one who proceeded in this way, even granting that it has ever been done, which is not known to have been the case with any recorded prodigy. We have already seen that both the mathematical and the psychological processes involved in mental multiplication differ considerably from those of written multiplication. Moreover, the two have quite distinct origins; mental arithmetic grows naturally and independently out of counting, written arithmetic out of more or less arbitrary rules learned from teachers and books. Any argument based on the methods of written multiplication, therefore, or on the difference in speed between the two dissimilar operations of mental and written arithmetic, must be accepted with considerable caution. The fact that on paper the computing and the recording are two distinct operations does not prove that in mental arithmetic memory and calculation are two separate faculties, and that when the numbers to be multiplied are very long, the difficulty is all on the side of memory and not at all on the side of calculation.

Even in Buxton's case, where the discrepancy between memory and calculating power seems as clear as anywhere, we cannot give the credit to memory, as opposed to calculation. Buxton's calculations, to be sure, whether long or short, were excessively slow; but so was his memory (in the sense of power of acquisition of figures): we are told that he comprehended even (simple) arithmetical questions "not without difficulty and time." [1] Like Bidder and many—perhaps most—other calculators, he began work at the left of the numbers, adding up the partial products as he went along, and not waiting until all were obtained. Moreover, we have seen fairly good reasons for supposing that he worked by a process of modified counting, rather than by multiplication proper, dividing the work into stages in his own peculiar way, but for the most part keeping in mind only one or two partial results, and using them in calculation as soon as possible. This means, however, that his processes in a long multiplication were not simply the same as in 2-figure multiplications, plus a tenacious memory; he really *calculated* with the *large* numbers, and in dealing with them doubtless used some special short-cuts, though far fewer than most other calculators. In short, he really was, in his way, a "great calculator," not a "little calculator" with a "big memory." He calculated with large numbers because they had an interest and a meaning for him which they have not for the ordinary man. The difference between the man who can unravel a half-page sentence of technical German and the one who can scarcely understand a two-line sentence is not primarily a matter of memory, but of interest and meaning; and the same holds good of the calculator who handles large numbers, as compared with the one who does not. Zaneboni had an unusual interest in memorizing figures for their own sake, and Inaudi had a highly developed figure-memory, yet we do not read that these men made a specialty of long multiplications;

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[1] *Gent. Mag.*, XXIV, 1754, p. 251.

their figure-memory as such and their figure-memory as subservient to their interest in calculation were quite distinct.[1]

We cannot agree with Binet,[2] therefore, that mental calculation combines the two distinct and independent elements of memory and calculation, of which memory is really distinctive in the case of the great calculators, the one characteristic in virtue of which they are inimitable and indefinitely superior to the rest of mankind. Inaudi, to be sure, was inferior to the department-store cashiers on short multiplications, because he had not practiced them as much as his rivals had; but his superiority in long multiplications is equally due to practice in calculation, not to superior memory as such. Some of his calculations were slower than those of a good computer on paper, others were faster; but the mental and even the mathematical processes are so different in the two cases that no value attaches to the comparison. His skill in memorizing long numbers was the result of practice, perhaps in the service of addition, perhaps for its own sake, but was not the secret of his calculation. That he remembered the 200 or 300 numbers used in one of his public exhibitions was due to his *interest* in the figures, on account of their connection with his calculations; where that interest was lacking, he could remember only a third as many figures in the same period of time. And how little connection his figure-memory had with his calculations proper is shown by the fact that when he exceeded the range of his practice in *calculation*, and passed from a 5-figure to a 6-figure multiplication, he required six times as long,—a result inexplicable, in a man who could retain 42 figures on a single hearing, if memory is the real secret, but much less surprising if practice in calculation is the important thing. In short, figure-memory is important in the psychology of mental calculation only in so far as it stands *in the service of calculation* and intimately bound up with it; to make a sharp distinction between the two, and lay the emphasis on memory as *opposed* to calculation, is to be led astray by a distinction of common-sense psychology whose usefulness has long since been outgrown.

(*b*) *Memory type*. We have seen that up to 1892, when Inaudi was shown to belong to the auditory type, it was generally taken for granted that all mathematical prodigies were visual. Of the two important calculators since discovered,

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[1] In the case of long additions, such as Inaudi's additions of 21-figure numbers, the distinction between memory and calculation may at first seem clearer than in multiplication. Actually, however, the two are, if anything, more closely connected in addition than in multiplication; for the calculation involved is so simple and automatic that it probably always begins during the process of memorizing the second number.

[2] *Op. cit.*, p. 194.

Diamandi is undoubtedly visual, and Zaneboni is so described by the authors of the article in the *Rivista di Freniatria*, though, as we shall see, his case is somewhat doubtful. Up to the present time, then, Inaudi has been supposed to be the only non-visual calculator on record; no one seems to have raised the question whether all the supposedly visual calculators actually were visual and not auditory. It is proposed here to make an attempt to answer this question; and while it will, of course, not be possible in all cases to reach perfectly definite conclusions, we can at least make a start in the right direction, and put the whole subject on a more satisfactory and critical basis.

First, however, it will be necessary to define our terms. It should be understood at the outset that no absolute line can be drawn between the two types. In ordinary life the most common type would probably be the "mixed" type. But it will be impossible, on the basis of the available data, to enter upon this question of the *general* memory type of the different calculators. The most we can hope to do is to determine to which type they belonged *in their mental calculations;* and while it is probable that this will usually coincide with their general type, the fact can by no means be taken for granted. Fortunately, however, it is much easier to decide in a narrow field than to decide the general question; and in calculation, especially, there are a number of fairly definite and reliable indications which will render the task somewhat easier than it would be in many other fields.

By a *visual* calculator, then, we shall mean one in whose mental calculations visual images of some sort play an essential part. Auditory elements will probably always be present besides, in greater or less degree; but if the visual elements play an essential rôle, we may call the calculator visual, since any more minute classification would be impracticable without more detailed information than we possess concerning the individual men. The visual elements may take various forms. The calculator may see numbers as dot-patterns, as Proctor did. He may have a number-form. Or he may see the figures of his calculations written or printed before him, more or less distinctly, but not necessarily in complete detail, since short-cuts and abbreviations may be connected with visual images as well as with any others. In other words, the visual image need not be a photographic reproduction of the complete written calculation; if it is essential, in however schematic and abbreviated a form, its presence is the mark of a visual calculator.

Inasmuch as written figures are the commonest form of the visual image,—Proctor is the only calculator known to have used dots in his actual calculations,—it is to be expected that in most cases the visual calculator's methods of calculation will not take shape until after he has learned to write figures. Since the visual image is simultaneous, while the auditory image is successive, it will be easier for a visual than for an auditory calculator to reverse his image, and call off a number backwards. Hence the ability to read off a number from memory *either* forwards or backwards will establish a presumption in favor of visual type. This is not a conclusive indication, however, since with practice an auditory calculator also could learn to reverse his numbers, especially where they were short. At the same time, the reversal is so much easier for a visual than for an auditory image that it must be considered as establishing at least a presumption in favor of the visual type, especially when corroborated by other indications.[1]

We may call a calculator *auditory* (or auditory-motor), on the other hand, when there is some definite reason for believing that visual images do *not* play an essential part in his mental calculation, or that motor tendencies are closely associated with it. Visual images need not be completely absent, especially in the early years of a precocious calculator; but if they were present at the start, we must have some reason for believing that they became unimportant as his proficiency increased. Similarly, the motor tendencies may, with practice, partly or wholly drop out; but in this case they will tend to return when new and unpracticed facts are attempted. In consequence of the *verbal* nature of the *counting* process in which natural mental arithmetic begins, it is hard to conceive of a visual calculator in whose operations words (and hence auditory images) do not play a part of greater or less importance; it is much easier, on the other hand, to conceive of an auditory calculator in whom visual elements are almost wholly absent, except in the form of an occasional chance by-play of associations which have no essential function in the calculation proper. If this is the case, however, it constitutes a presumption that any given calculator is auditory unless evidence exists to the contrary, and throws the burden of proof upon the visual side.

The most definite indications of auditory type are as follows: (1) A tendency to articulate during calculation, especially in relatively unfamiliar operations; this may be regarded as practically conclusive evidence of auditory (or auditory-motor) type. (2) The absence of any indication of visual elements in the prodigy's mental calculations. This argument, however, is of no practical weight except in cases where we have a fairly full account, from the prodigy himself, of his methods of calculation. Where this is the case, it is fairly safe to assume that at least some of the various indications of visual type will be present in the narrative if the calculator is really visual. Colburn and Bidder are the only men who have left us such narratives, and in neither case do we find any reference to visual processes; hence a strong presumption exists that both these men were auditory. (3) General resemblance to known auditory calculators. This is not a very satisfactory indication, in general, unless the resemblance is close in several important respects; but in certain cases, owing to the meagreness of the sources of information, it is almost the only indication we have. It is more reliable, of course, if it covers several indications which, though perhaps each inconclusive by itself, may have a cumulative weight.

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[1] Curiously enough, however, we have no specific record of the possession of such a power of reversing numbers by either Diamandi or the younger Bidder, the only two calculators for whom we have perfectly definite and unequivocal evidence of visual type; though it is practically certain that Diamandi, at any rate, possessed the power in some degree, since after learning a square of figures by rows from left to right he could, with little difficulty, repeat them in ascending or descending columns.

Let us now examine the evidence in the case of the different individual calculators, in each group taking those cases first where there is least room for difference of opinion.

As *visual* calculators we may name at once the younger Bidder and Diamandi, both of whom possessed number-forms and used cross-multiplication; the younger Bidder, moreover, made the not very profound observation that he could "conceive no other (non-visual) way possible of doing mental arithmetic." Diamandi took up mental calculation after leaving school, at the age of 16, and hence after learning to read and write; the same is probably true of the younger Bidder, though inasmuch as his number-form shows the influence of the clock in the circular arrangement of the numbers up to 12, he may possibly have learned numbers visually from this source before he learned the alphabet.

Whether *Dase* was of the visual type it is difficult to say. He may have learned to read and write before he began to calculate,[1] though this is not certain; he could repeat numbers backwards with great ease; he could learn a number of 12 figures almost at a glance; and his skill in rapid visual counting was remarkable. All these indications point in the visual direction, yet they are by no means as satisfactory as the proofs in the other two cases. We have already seen that Dase stands in a class by himself, and that an enormous amount of practice is presupposed by his feats, whatever his memory type; it is not impossible, therefore, that he was auditory, since with sufficient practice an auditory calculator could learn to reverse numbers readily and to count objects at a glance. Robert Houdin and his father, we know, acquired by practice a considerable power of rapid visual counting. Moreover, it does not follow that a man who by practice has developed a power of visualization in this field will likewise use visual images in mental arithmetic, especially since the precise amount of visualizing power needed for Houdin's feats is not at all easy to determine. But in order to avoid one-sidedness we may give the visual theory the benefit of the doubt, and call Dase a visual calculator, bearing in mind, however, that the evidence is by no means as satisfactory in his case as in the case of Diamandi or the younger Bidder.

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[1] He attended school at the age of 2½, but attributed his skill in calculation to later practice and industry rather than to his early schooling.

The case of *Zaneboni* is likewise somewhat doubtful. He began to calculate at the age of 12, after learning to read and write, and used written lists in some of his memory feats; he also seems to have made more or less use of visual images in answering questions concerning railway distances and the like. He could repeat a memorized 256-figure number almost as readily backwards as forwards. It is not at all clear, however, to just what extent he used visual images in his mental calculation proper, as distinguished from figure-memory proper; and in the experiments of Guicciardi and Ferrari there are some indications which clearly show a leaning in the auditory-motor direction. If we were discussing his memory-type in general, instead of his type simply in calculation, we should undoubtedly be safe in describing him as "mixed"; but the definitions we have adopted of the terms "visual calculator" and "auditory calculator," do not admit any intermediate type, since we have agreed to call a calculator visual if visual images play any essential part in his calculations. A detailed examination of the available data concerning his type as a calculator would take up far more space than its importance would warrant, and even then would leave us in doubt just where to place him. It seems wisest, therefore, to give the visual theory once more the benefit of the doubt, and call Zaneboni a visual calculator, despite the inconclusiveness of the existing evidence. We find so far, then, four visual prodigies,—the younger Bidder, Diamandi, Dase, and Zaneboni,—of whom only the first two, however, are above suspicion.[1]

As *auditory* calculators we may name at once Inaudi, Ferrol, who in ordinary life was an abnormally poor visualizer, Safford, who talked to himself when originating new rules, and the present writer. The cases of certain other calculators who were almost certainly auditory require some discussion, since at first sight the evidence appears to be conflicting.

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[1] The name of Proctor has been omitted from this list, owing to the absence of information concerning the extent of his calculating power; there is no doubt, however, that his calculations, as far as they went, were visual in the sense already defined.

That *Bidder* was of auditory type is shown by the following considerations. (1) He learned to count and calculate before learning to read and write, and constantly emphasizes the importance of this fact in his lecture before the Institution of Civil Engineers. After learning to count, he taught himself the multiplication table to 10 x 10 by means of rectangles of shot; but he expressly speaks of *counting*[1] the shot after thus arranging them, not of seeing a picture of them, or of grouping them in visual patterns. Again, when he recommends this method of teaching mental arithmetic,[2] he lays stress on arranging the actual objects and then counting them, not on seeing such vivid mental pictures as would render the presence of the actual objects superfluous. Hence we are warranted in assuming that if Bidder had visual images of his shot and marbles at first, they played no part in his actual mechanism of mental calculation, and later dropped out. If this is the case, then the use of pebbles and the like in childhood is not, as it would at first seem, a proof of visual type.

(2) Bidder's reiterated emphasis on the *teachableness* of mental arithmetic, by his methods, to any person of average ability,[3] is itself a fairly good indication of his auditory type. Any one can learn to count in auditory terms, but not every one can learn to visualize numbers, to "have a good imagination" for them. If visual aids played any essential part in his mental calculations, then, we should expect him to stipulate that the mental arithmetician must possess a certain minimum of imagination. Bidder's son, it will be remembered, who was of strongly visual cast, could "conceive no other way possible of doing mental arithmetic." The elder Bidder, on the other hand, can discover in himself[4] "no particular turn of mind, beyond a predilection for figures, which many possess almost in an equal degree with myself."

(3) There is a notable absence of visual metaphors in Bidder's descriptions of his calculating operations, and a strong insistence on the *successive* character of the processes involved; whereas a visual calculator, in an account so full and clear as Bidder's, could hardly have avoided at least a few visual comparisons, and would not have laid stress on the serial, one-thing-at-a-time character of the processes. Bidder speaks[5] of

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[1] *Proc. Inst. C. E.*, XV, p. 258.

[2] *Ibid.*, p. 278.

[3] *Proc. Inst. C. E.*, XV, pp. 252, 253, 256, 261, 278.

[4] *Ibid.*, p. 253.

[5] *Proc. Inst. C. E.*, XV, p. 254.

"registering" the numbers in his memory, not of seeing them; in mental calculation "every mental process must be analogous to that which is indicated in working out algebraical formulæ," and, "no one step can be omitted." In reading the page of a book, "every letter of that page passes in review through the mind"; note the abstractness of the phrase here, where a visual figure would be almost inevitable for a man of visual type. Ideas of numbers are best impressed on the mind[1] "without any reference to symbols"; written figures are[2] "unmeaning symbols." As soon as we attempt to go beyond 3-figure multiplications to those of 4 figures, "another idea must be seized by the mind", namely, the idea (word) "thousands," and "this increases the strain on the registering powers of the mind."[3] In multiplication, one fact and only one is to be kept in mind at each stage.[4] Bidder would[5] "despair of any great success in the pupil's progress in the science of arithmetic if he did not commence before he knew anything of symbols, and if his first conception of numbers was not derived from their real tangible quantity and significance." Here, as frequently throughout the paper, the written symbols (visual and simultaneous) are contrasted with the "significance" of the numbers, *i. e.*, their place in the (successive and auditory) series of numbers used in counting.

In the one place (p. 255) where Bidder does use a visual comparison, it is the hackneyed one of a flash of lightning—an expression which has passed so completely into the every-day vocabulary that its original figurative force is seldom distinctly realized by those who use it. Bidder employs the phrase simply to show the rapidity and clearness with which needed numerical ideas come into his mind; this no more indicates visual type (where the counter-evidence is so strong) than the familiar phrase "like a flash" proves that things "understood" in that way are "seen" in visual terms. In fact, the verb "see" and the commoner stereotyped visual metaphors ("clear," "like a flash," etc.) are so firmly established as synonyms for "understand," etc., that, unless corroborated by more definite evidence, they prove nothing at all concerning the memory type of any one who uses them.

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[1] *Ibid.*, p. 263.

[2] *Ibid.*, p. 261.

[3] *Proc. Inst. C. E.*, XV, p. 263. Inaudi, like Bidder an auditory calculator, always keeps in mind the names (thousands, millions, billions, etc.) of the different periods of numbers, whereas a visual calculator would be much more likely to depend simply on his visual memory of position, and drop out the names of the periods as useless, like the words "put down 3 and carry 2," etc.

[4] *Ibid.*, pp. 260, 263.

[5] *Ibid.*, p. 279.

There can be little or no doubt, then, that Bidder was an auditory calculator. Scripture's sole evidence[1] of visual type in Bidder's case consists in wrongly attributing to him the younger Bidder's remark about the only conceivable way of doing mental arithmetic. The only other apparent evidence that Bidder was a visual calculator is the fact that he had good powers of visualizing diagrams and the like.[2] But we are not trying to determine Bidder's memory type in general, but only his type as a calculator; and the evidence in the latter field is practically conclusive in favor of auditory type. If we knew absolutely nothing else about Bidder's memory type in general except that he was a good visualizer of diagrams, that fact might establish a very slight presumption in favor of visual type even as a calculator; but in the presence of more definite evidence the fact is quite irrelevant here, except in so far as it shows that he was not, like Ferrol, of *extreme* non-visual type outside of calculation.

What, now, of *Colburn's* memory type? In Gall's account[3] we are told that "in calculations at all complicated, he [Colburn] is often heard to multiply, add, or subtract, aloud, and with incredible rapidity." In the *Philosophical Magazine*,[4] too, we find a reference to the "motion of his lips while calculating," and this we may take as practically conclusive evidence of auditory (auditory-motor) type. As corroborative evidence we may point to the close resemblance of the three calculators Bidder, Safford and Colburn, all of whom learned to calculate before learning to read and write, and showed a marked preference for a class of problems (evolution and factoring) not cultivated to an equal extent by any other recorded calculator. All of them began multiplication at the left, and in general had the same methods of calculation. Two of them, Bidder and Safford, we have already found to be auditory, and in Colburn's case the reference to the motion of his lips is strongly in favor of the auditory theory. Moreover, we find in him the same nervous contortions which are recorded in the case of Safford, and, to a less extent, of Inaudi,[5] both auditory (-motor) calculators.

In Colburn's case, as in Bidder's, there is one piece of apparent evidence of visual type. We read in Gall's account[6] that Colburn "was asked how he made his calculations. He answered, that he saw them clearly before him." But, as we have already seen, the use of the words "see" and "clearly" is not a satisfactory proof of visual type, especially when strong and definite evidence points the other way. If Colburn had depended on Proctor's dot-patterns, that fact ought to be evident from his account of his methods in the *Memoir*, whereas we find there no reference to visual images. That Colburn saw written figures is hardly probable, since it was only in London, when almost at the height of his calculating power, that he learned to read and write. Furthermore, when the 1811 article was written on which Gall's account is based, Colburn was only 6 years old, and hence would naturally use concrete language (visual metaphors) rather than abstract language,[1] whatever his memory type. In the light of all the available evidence, therefore, it seems safe to call Colburn an auditory calculator, though it is of course just possible that at first he had visual associations also, which later may have disappeared more or less completely from his calculations.

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[1] *Op. cit.*, p. 57.

[2] *Spectator*, LI, 1878, p. 1634.

[3] *Op. cit.*, V, p. 85.

[4] Vol. XL, 1812, p. 122.

[5] Binet says of Inaudi (*op. cit.*, p. 37), "Pendant les calculs, il fait différents gestes, tics sans importance et du reste très variables."

[6] *Op. cit.*, V. p. 87, quoting from *Med. and Phil. Jl. and Rev.*, 1811; Scripture, quoting the remark, gives the reference as p. 22 of that journal.

Let us next consider the case of *Buxton*. His ability to repeat numbers either forwards or backwards establishes a slight presumption, as we have seen, in favor of visual type. On the other hand, there is no lack of presumptive auditory indications and points of resemblance to auditory calculators. Buxton not only learned to count before learning to read and write; he never learned to read and write at all. He not only, like Inaudi, retained the names of the different periods (thousands, millions, etc.) in thinking of numbers, but invented peculiar names of his own for the earlier periods of very large numbers. Like Inaudi, again, he began calculating by "naming the several figures distinctly one after another, in order to assure himself of the several dimensions [the problem was in mensuration,] and fix them in his mind."[2] Like Bidder, Colburn, Inaudi and Safford, he began at the left in multiplication; whereas the two unmistakably visual calculators, Diamandi and the younger Bidder, are known to have used cross-multiplication. He retained a marked fondness for that counting in the simple series of natural numbers with which the auditory calculator always begins; at the theatre, for instance, he counted the exits and entrances of the different characters, the words spoken by each, the number of steps taken in the dance, and the like. His mental record of all the free beer he had had from the age of 12 must have been at least partly in verbal (auditory) terms. His skill in estimating areas by pacing them does not indicate any visualizing ability, but rather a step of uniform and known length and an ability to count and calculate. Like Inaudi and a few other calculators, none of them known to be visual, he could carry on a conversation while computing. And when a new style of problem (cube root) was proposed to him, he was heard to mutter to himself, after puzzling over it for some time, that he would master it yet.[1]

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[1] Assume for the moment that a child of 6 is an absolutely non-visual calculator, and is asked to describe his methods; what language has he at command *but* the visual metaphor of "seeing clearly"? He does not yet distinguish words from thoughts, hence cannot say, as a grown man might, that he "hears" his calculations, or "speaks" them mentally. The psychology of a young child, like popular psychology in general, inevitably tends to explain everything in visual terms.

[2] *Gent. Mag.*, 1751, p. 61.

The vast extent of some of Buxton's calculations, to be sure, suggests Dase, the only other calculator known to have gone beyond 15-figure multiplications; but, even granting that Dase was a visual calculator, the value of this analogy is destroyed by the fact that whereas Buxton required two months and a half for a 39-figure multiplication, Dase performed a 40-figure multiplication in 40 minutes. And of course if Buxton had possessed anything like Dase's power of instantaneous visual counting, such trivial occupations as counting words and steps could scarcely have retained any interest for him. On the whole, then,—since the ability to repeat numbers backwards is the only indication of possible visual type, and since we can easily explain that ability, in a man whose mental processes were all so slow, by supposing that he became interested in reversing numbers and practiced the feat with his usual perseverance,—it seems fairly safe to call Buxton an auditory rather than a visual calculator.

The remaining calculators may be dismissed somewhat more briefly, since in the absence of more definite information than we have it is only possible to indicate probabilities as established by comparison with better known prodigies. *Fuller* so closely resembles Buxton that if we call the one auditory, we may assume that the other was likewise. *Mondeux* has various points of resemblance with Inaudi on the one hand, and with Bidder on the other, both of whom we have found to be auditory, hence Mondeux also may be placed in this class. *Ampère*, like Mondeux and Bidder, used pebbles in his early calculations, an indication which we have found to be no disproof of auditory type; like them, again, he learned to count at an early age,—earlier, in fact, than either of them,—before learning to read and write. A presumption therefore exists that he, too, was auditory. *Gauss* began earliest of all, before he was quite 3, if we accept his own account, as it seems safe to do in the case of a man of his scientific eminence, who had so little need to exaggerate his own ability; moreover, the fact that at the gymnasium he mastered the classical languages "with incredible rapidity" suggests auditory rather than visual characteristics. Concerning the *minor prodigies* it would hardly be safe to speculate; we have seen, however, that many of them learned counting before reading and writing, and this establishes in these cases a slight presumption of auditory type.

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[1] *Gent. Mag.*, 1753, p. 557. The exact wording of the passage is, "after some time, he said to himself *there were nooks in it* [the problem], *but he would sift them about*."

These conclusions, it will be seen, involve a radical departure from the current views. Scripture names three calculators—Buxton, Colburn, and Bidder—as using visual images; and while he admits the absence of specific information in the case of other calculators, he thinks it safe to assume that visual images played a considerable part in all cases. We have seen, however, that the evidence in each of the three cases named is either based on a misconception, or counterbalanced by stronger evidence of auditory type. On the other hand, two of the prodigies in Scripture's list, Dase and the younger Bidder (who obviously should have been named, since he and not his father was responsible for the remark quoted by Scripture), show some evidence of visual type, and of the three others—Inaudi, Diamandi, and Zaneboni—since described, Diamandi is unmistakably visual, while Zaneboni is rather doubtful. Apart from these four men, we find in almost every case not only no evidence supporting the visual theory, but at least some evidence, of more or less weight, favoring the auditory theory. In other words, if the conclusions here reached are sound, mental calculation is primarily an auditory operation, a matter of verbal associations, and in the majority of cases no appreciable aid is rendered by visual associations. In fact, while it would perhaps be going too far to say that the "natural" calculator is always the auditory calculator, and that the visual calculator, just in so far as he *is* visual, is an "artificial" calculator, such a statement would be very much nearer the truth than the current view that mental calculation is primarily a visual operation.[1]

In case this view seems unduly radical, however, it must be remembered, in the first place, that we are dealing only with mental calculation as such, leaving quite open the question of general memory type; and in the second place, that we have adopted a very narrow definition of the two types. By calling a calculator auditory we do not imply that visual images are by any means wholly absent from his mind during calculation, but only that they are not uniformly present, and that their absence is not an embarrassment, or their presence a material aid, to the calculator. Probably in most cases what we have called chance visual associations will enter in to a greater or less extent, especially where the numbers are presented to the calculator in written or printed form. And it may conceivably happen that a man who is, in general, predominantly visual may be auditory, in this sense, as a calculator. Yet after all qualifications have been made, the fact remains that mental calculation, as we have seen again and again, naturally takes its rise from counting, which is essentially a verbal process. However widely the extent and relative weight of visual and auditory elements may vary, it can hardly conceivably happen,—certainly does not happen in any known case,—that the verbal elements play no part whatever in the calculating process; whereas the function of the visual elements may well be reduced practically to the vanishing point. And the sooner we get rid of the old idea that mental calculation is in essence the same as written calculation, but with a mental instead of a written tablet, on which the faculties of memory and imagination inscribe the figures of the written calculation in perfect order and complete detail, the sooner we shall begin to understand the psychology of mental calculation.

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[1] Diamandi, we have seen, learned mental calculation only after leaving school, and used cross-multiplication, which is not a "natural" method. The younger Bidder used cross-multiplication and a mnemonic system, and may have had no "natural" gift for mental calculation at all, but simply have taught himself by deliberate practice, in imitation of his father, or by his father's suggestion or guidance. The cases of Dase and Zaneboni, cannot profitably be discussed from this point of view in the absence of fuller information than we possess.

Summary. The results of our study of the mathematical prodigies may be summarized as follows:

(1) In Part I is presented a fairly complete list of the more important prodigies on record, with those data in each case which shed most light on the nature and development of the calculating power. An effort is also made to correct several errors which have crept into the literature, and to bring out a few points whose significance has been overlooked; so that the present account, it is hoped, will be found reasonably complete and reliable. In particular, the case of Zerah Colburn has been entered into in some detail; this man has certainly received less than his due from both Scripture and Binet.

(2) In Part II is described a new case, that of the present writer. The calculating power is in this case very slight, and in itself unimportant. It has been described at some length, however, to bring out the naturalness of the "precocity" involved, the gradualness of the development of the calculating power, the important part played by counting,—first in the series of natural numbers, and later in the series of multiples and powers of various numbers,—the general character of the numerical properties brought to light in this way,—properties

which awaken the interest of the calculator, furnish the motive for continued practice, and shorten the labor of calculation, rendering the whole process self-sufficient and independent of outside aid,—and, to some extent, the nature of the psychological and mathematical processes and short-cuts used in this particular case. The part played by "2-figure endings" has been entered into in some detail, as shedding light on a class of problems—evolution and factoring—which have often puzzled students of the subject, owing to the surprising rapidity with which some of the prodigies have solved them. In none of these respects is the material here presented wholly new. Most of it, perhaps, could be deduced from a careful study of Bidder's case. At the same time, it remains true that the significance of these facts has *not* been fully brought out by previous investigators; hence it has seemed worth while to dwell on them here at greater length than would otherwise be necessary.

(3) The data thus collected have been studied in Part III, and conclusions drawn under several heads. Attention has in the main been confined to the "natural" calculators who develop spontaneously, at least in the first instance, without external aid from books and teachers; as distinguished from the "artificial" calculators who use external aids from the start. Huber's blind Swiss is perhaps the only "artificial" calculator here considered; but the distinction must not be too sharply drawn, since several of the others have made use of "artificial" methods, such as mnemonic systems. A calculator may begin in the "natural" way, but later make use of "artificial" methods besides, in order to extend his calculations further, as Gauss did in his use of logarithms; if the younger Bidder had any "natural" gift to start with,—a question we must here leave open,—he belongs to the same class with Gauss in this respect. In general, it is obvious that skill in calculation attained by using these "artificial" methods, either from the start or at a later stage, constitutes no special problem for the psychology of mathematical precocity, belonging rather to the psychology of deliberately practiced operations in general, and hence need not be discussed in such a study as the present.

Into the question of *heredity* in mental calculation we have not attempted to enter, not only because of the scarcity and uncertainty of the data, but because such general terms as heredity and environment do not carry us far in the study of any special function like mental calculation.

Precocity in calculation, we have found, is natural and normal; not only is the popular amazement over it groundless, but there is no need even to regard it as "remarkable." Owing to the origin of mental calculation in ordinary counting, and the complete independence and self-sufficiency of mental arithmetic, mere mathematical precocity falls in a different class from musical precocity, and still more from the all-round precocity shown by such men as Ampère and Macaulay. If for any reason the mathematical prodigy loses his interest in calculation, or the opportunity to practice it, his power is likely to diminish or eventually to disappear; in this respect mental calculation is like piano-playing, or any other highly specialized activity dependent on long practice.

Skill in mental calculation is, owing to the isolation of mental arithmetic already noted, independent of general *education;* the mathematical prodigy may be illiterate or even densely stupid, or he may be an all-round prodigy and veritable genius. Furthermore, mental calculation is entirely independent for *mathematical* ability and education; the calculator may never rise above the counting stage, or may acquire merely arithmetical skill, or may develop a keen insight into algebraic relations, or even, like Safford and Gauss, a marked aptitude for higher mathematics. Hence it is not helpful to classify the mathematical prodigies either by general education or by subsequent mathematical development. Indirectly, however, ignorance favors a high development of the calculating power, by preventing other and more important interests from taking its place. Where the power *is* retained in spite of the widening of interests, its practical value may become considerable, especially to the mathematician, the lawyer, or the engineer.

Although mental arithmetic naturally develops out of *counting*, the fundamental operation is *multiplication*. This operation may remain permanently in the counting stage, *i. e.*, may proceed without the use of a memorized multiplication table; usually, however, a multiplication table up to 10 x 10 is used. Many of the prodigies begin at the left in both multiplication and addition. Proctor describes in his own case a method by the aid of visual dot-patterns, but no other calculator is known to have followed this method. Two of the visual calculators, Diamandi and the younger Bidder, used cross-multiplication. The theory has been proposed that a large multiplication table, perhaps to 100 x 100, is used by some of the prodigies; but there is no evidence that any of them actually did use such a table, and even Dase's feats are explicable without presupposing it. Problems in square and cube root (especially of exact squares and cubes) and factoring are favorites with some of the calculators; in those cases where the answer is given "instantly," the simple properties of 2-figure endings are used.

Various mathematical and psychological *short-cuts* explain the speed attained by some of the prodigies in their menta operations. Problems done "instantly" are either very simple, or else are solved by guess and trial, with the aid of little tricks and properties readily discovered by the calculator. Many algebraic problems are thus solved, and the same is true of square and cube roots of perfect squares and cubes, as we have just seen.

The distinction which is so often made between *memory* and *calculation*, with the implication that the great calculator is simply a little calculator with a big memory, using the same methods as his lesser rivals, is misleading; the process is always (in the "natural" calculators) a true calculation, and memory for figures is important only in so far as it stands in the service of calculation.

Finally, many of the calculators heretofore supposed to be of visual *memory type* turn out, on closer examination, to belong to the auditory (or auditory-motor) type, at least in calculation; and, in general, since counting is essentially a verbal process, the calculator who begins from counting, before he learns to read and write, will usually belong to the auditory type, and will make relatively little use of visual images in his actual calculations. At least two of the "major" prodigies, however, and possibly four, belong to the visual type.

APPENDIX I.

NOTE ON ZERAH COLBURN.

Scripture, at the beginning of his treatment of Colburn (*op. cit.*, p. 11), says: "Autobiographies do not always furnish the most trustworthy evidence in regard to the man himself; when, moreover, the author is convinced that he is nothing less than a modern miracle; and, finally, when having had no scientific and little literary education, he at a later date writes the memoirs of his youth, we are obliged to supply the lacking critical treatment of the narrative." A little earlier (p. 8) he tells us that Gauss, "if he had had the misfortune to have been gifted with nothing else [than his calculating powers], ... might even have proclaimed himself in the Colburn fashion, as a miraculous exception from the rest of mankind." Again (p. 16), "It is to be remarked that Colburn's calculating powers, such as they were, seemed [*sic*] to have absorbed all his mental energy; he was unable to learn much of anything, and incapable of the exercise of even ordinary intelligence or of any practical application. The only quality for which he was especially distinguished was self-appreciation. He speaks, for example, of Bidder as 'the person who[1] approached the nearest to an equality with himself[2] in mental arithmetic.' Again, 'he thinks it[1] no vanity to consider himself first in the list in the order of time, and probably first in the[2] extent of intellectual power.'"

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[1] The words "in the writer's judgment" are here omitted by Scripture without the customary sign of omission. (*Memoir* p. 175.)

[2] Colburn's word here is "him," not "himself." (*Loc cit.*)

In similar strain Binet says (*op. cit.*, p. 9). "L'histoire de Zerah Colburn serait extrêmement intéressante si elle reposait sur des documents dignes de confiance; il n' en est malheureusement pas ainsi. Le principal document qui reste de lui est son autobiographie, et comme il s'est exhibé dans des représentations publiques, et qu' il parle de lui-même avec une vanité insupportable, on peut supposer à bon droit que cette biographie est une réclame." And again (pp. 10-11), "Colburn a passé pour un individu d' une intelligence médiocre, et crevant d' orgueil; sa biographie en donne mille preuves naïves, et il affirme à plusieurs reprises qu'on doit le considérer comme la plus grande intelligence de la terre."[3] Other passages of a similar sort might be quoted from both Scripture and Binet, but those above given are sufficiently typical.

These statements, it will be seen, are plain and unqualified. If they are true, Colburn was a man of little or no education, incapable of ordinary intelligence, utterly unqualified to write a historical document; and his *Memoir* is historically unreliable, an exhibition of intolerable vanity, proclaiming him as a modern miracle, the greatest intellect that ever lived, etc., in the most naïve fashion. In a word, these writers portray Colburn as a sort of Buxton proclaiming himself as a Bidder or a Gauss. Let us now examine the facts, and see how far they bear out this interpretation.

An account of Colburn's education has been given in Part I of the present paper; from that account it will be seen that Scripture's statements are, to say the least, somewhat sweeping. We are interested in the matter, however, only in so far as it concerns Colburn's qualifications for writing a reliable account of his own life; and of those qualifications the *Memoir* is itself the best test. After a careful reading, the present writer finds it internally consistent, and to all appearance painstaking and trustworthy, with no aim other than to set the facts in their true light; and externally he has found only one or two slight discrepancies between it and other available documents, none of them at all comparable to some of the inaccuracies we have found in the article on *Arithmetical Prodigies.* We may therefore dismiss this general charge, until it is backed up by specific instances of Colburn's alleged incompetence, and turn our attention to the charge of naïve vanity and self-advertisement brought against Colburn by his critics. This is the real ground on which they have sought to discredit the *Memoir.*

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[1] The original reads, "it is no vanity", etc. (p. 176).

[2] The word "the" here is not in the original (p. 176). Thus Scripture here gives us four misquotations in three lines,—an eloquent commentary on his method of supplying "the lacking critical treatment of the narrative"! His "critical treatment" of the Bidder family has already been discussed.

[3] Binet admits (p. 1) that his historical account is superficial and second-hand, and (p. 2, note) that he has borrowed largely from Scripture; we shall therefore not, as a rule, take account of his misstatements, though they are not few. Scripture's account, however, professes to be based on the original sources, and so may properly be held accountable for its use of those sources.

There are three classes of passages in the *Memoir* on which a charge of undue self-appreciation may be based. (1) Those in which Colburn speaks of his calculating power as a gift of God, and the like. (2) His section (p. 173 ff.) on "Other Calculators," in which he attempts to estimate his own place among the calculators he had met or heard of. (3) A few scattered passages in which his language is, if not vain, at least in appearance a little unguarded. Let us consider the passages in this order.

(1) Passages of the first class may be very briefly dismissed. Colburn was brought up with eighteenth century ideas of a personal Creator, and at the time of writing the *Memoir* was a Methodist minister. For him, therefore, it is the plainest and most matter-of-fact statement possible to say that his power is a "gift of God," and unless the context shows undue pride in that gift, no charge of vanity receives the slightest support from such statements. Now actually Colburn uses these religious expressions only in this matter-of-fact way; he nowhere boasts of his gift, but, on the contrary, is frankly puzzled as to the Divine purpose in bestowing it, since it remained a mere "freak of nature," so to speak, and contributed neither to his material nor to his spiritual success. (*Cf. Memoir*, pp. 165-6.)

(2) We may now examine those parts of Colburn's section on "Other Calculators" (pp. 173-8) which bear on his own estimate of himself. The section begins with an account of his early nervousness, and concludes with some generalizing reflections, so that we may, without unduly crowding our space, quote practically in full the relevant parts of the section (pp. 173-6).

"The inquiry has frequently been made whether the writer ever became acquainted with any other persons who were endowed with a gift of mental calculation similar to himself. He thinks not, as to extent of solution." Here follows an account of what he had heard concerning Buxton, but with no attempt to compare himself directly with Buxton.

"The Countess of Mansfield called upon Zerah Colburn, while he was first exhibited in London, and alluding to the singular gift he possessed, stated that she had a daughter, Lady

Frederica Murray, who was about his age, and gave indications of superior skill in figures. He was afterwards invited to call at her ladyship's residence, and found the young lady did possess a certain degree of mental quickness uncommon in her sex and years. But her elevated rank, and the necessary attention to those pursuits which were more in accordance with her station in life, probably prevented her attending to that endowment. She was afterwards married to Colonel Stanhope, and dying young, her widowed husband, after the lapse of a few years terminated his existence by suicide.

"The person who in the writer's judgment approached the nearest to an equality with him in mental arithmetic, was a youth from Devonshire county, in England, named George Bidder. This person did not begin to excite attention until after Zerah had retired from public exhibition in London, sometime in the year 1815. Bidder was at that time ten years old.[1] Having never had any acquaintance with him, the author cannot speak correctly of the manner in which his talent was first communicated and exhibited.

"The only thing he ever heard on this point was, that his father being engaged in some difficult sum, George answered it at once; that in view of his unexpected readiness, he was put to school, and considerable pains were taken to train him for exhibition. This however may be as incorrect as some of the stories in circulation relative to the subject of this memoir. It is certain, however, that in London he [Bidder] never received that general patronage which his predecessor enjoyed.[2]

"Some time in 1818, Zerah was invited to a certain place, where he found a number of persons questioning the Devonshire boy. He [Bidder] displayed great strength and power of mind in the higher branches of arithmetic; he could answer some questions that the American would not like to undertake; but he was unable to extract the roots, and find the factors of numbers. The last time that the writer was in Edinburgh, he was informed that the lad was in study, under the patronage of a Scotch nobleman.

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[1] Bidder was not ten years old until 1816, so that Colburn, writing from memory some fifteen years or more after the event, has made a mistake of a year in one of these figures.

[2] If this sentence exhibits vanity at all, the vanity is hardly of the naive sort of which Colburn's critics have accused him. More probably, however, he states the simple fact. The Colburns had by this time worked the London field over pretty thoroughly, and the public must have had a genteel sufficiency of calculating prodigies. Even if we assume that Bidder's father was as assiduous as Colburn's in efforts to raise money from the nobility, which is doubtful, the generosity of the noble lords may well have faltered a trifle after Colburn had both worn off the novelty of the thing and collected all the subscriptions they were willing to give. At any rate, this passage is hardly as unequivocal a proof of vanity as we need to substantiate the sweeping charges of Scripture and Binet.

"At different periods, Zerah Colburn has heard of a number of persons, whose uncommon aptness in figures rendered them subjects of astonishment to others. He thinks it is no vanity to consider himself first in the list in the order of time, and probably first in extent of intellectual power.[1] It would be very easy to indulge in speculations in regard to the increasing number of persons thus endowed; but speculation avails little in so exact a science as mathematics, and would profit nothing on the present occasion. It is his opinion that should a similar case occur again," sufficiently promising, his education should be made a matter of public interest, etc. "It then would be seen more clearly than in any other way what was the object of the gift, and if a valuable help is therein concealed, it would be made public, and thousands might share in its advantages." The remaining paragraphs of the section on "Other Calculators" consist of general reflections which do not here concern us.[2]

Colburn thinks, then, that he has never met any one with a gift of mental calculation similar to himself, as to extent of solution; that Bidder, in his (Colburn's) judgment, came the nearest to being such a person, and was superior in several respects, but was unable to extract the roots and find the factors of numbers; and that among the calculators who have been subjects of public astonishment (*i.e.*, among professional calculators, hence not including Buxton), it is no vanity to consider himself first in the order of time, and probably first in extent of intellectual power (extent of solution). Such is Colburn's estimate of himself as a calculator.

Undoubtedly Colburn was mistaken; Bidder far excelled him, even at the time of their meeting in 1818. But we cannot leave the matter thus; in order to decide whether Colburn was led to this conclusion by vanity, we must examine the grounds he gives for it. Bidder, he tells us, displayed great strength and power of mind in the higher branches of arithmetic, and answered some questions he (Colburn) would not like to undertake; but Bidder "was unable to extract the roots, and find the factors of numbers." Now Colburn was not much of a mathematician, but one thing he did know, from the mathematicians who had examined him: up to his own time, no one had discovered any general method of finding the factors of numbers. Colburn himself had a new and original method of performing this operation very rapidly for numbers up to 6 or 7 figures, and of finding almost instantly the roots, of exact squares and cubes. He could not be expected to understand that this method (by 2-figure endings) was really trivial; he *did* know that he could solve these problems, by an original method, and that eminent mathematicians were more amazed at this feat than at any other in his repertoire. Shall we blame him, then, for considering himself superior as a calculator to any one who simply excelled him in straight arithmetical operations, and that, too, at a time when he had given up public exhibitions and lost not a little of his former skill? Colburn could appreciate his own feats, but could not adequately appreciate Bidder's compound interest method, for example; he gives Bidder full credit, however, for "great strength and power of mind in the higher branches of arithmetic," and for defeating him in the competitive test in other directions.

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[1] This is the only passage the present writer has found that even remotely supports Binet's assertion that Colburn "affirme à plusieurs reprises qu'on doit le considérer comme la plus grande intelligence de la terre." It need hardly be pointed out that by "intellectual power" in the present context Colburn means simply "power of calculation." Note the similar references to the "mental quickness" of the daughter of Lady Mansfield above, and to Bidder's "strength and power of mind."

[2] From these extracts the reader may judge for himself both of Colburn's style and of his historical ability or inability. In these respects, as well as for the purpose of illustrating Colburn's attitude toward his own gifts, they seem to the writer to be quite typical of the whole book.

Viewed in this light, the passage implies simply that Colburn was honestly mistaken in spite of a sincere effort to face the facts impartially. The charge of vanity receives no support from this part of his book, the one part above all others where vanity ought to show itself. The laudatory account of Bidder in the London paper, on the other hand, can be explained either by a better realization of the difficulty of Bidder's feats, particularly that of solving compound interest problems, by faulty memory on the part of the reporter or his informant, or even by simple patriotic partiality to the English boy. The War of 1812 was still fresh in the public mind, and love for persons and things American was not strong. Since it is not until 1819, the year after the meeting between Bidder and Colburn, that we find any record of Bidder's solving problems in square or cube root, there is no reason to dispute Colburn's statement that in 1818 Bidder had not discovered the methods he afterwards applied to it. So far, then, Colburn is completely vindicated; the charge of vanity rests on misinterpretation and on a failure to take account of all the circumstances in the case.

(3) The third class of passages need not long detain us. Two of them we have already examined; those, namely, in which Colburn says that Bidder "never received that general patronage which his predecessor enjoyed," and speaks of him-

self as "first in extent of intellectual power." After what has been said, a fair-minded critic will hardly attach serious weight to either of these passages. The only other one which seems to offer any chance for misunderstanding, even to a superficial reader, occurs on p. 63. Colburn is describing the plans for the projected book for which a committee of his admirers had been attempting, not very successfully, to collect subscriptions. The book was to be "a quarto volume, with a portrait; printed on the best paper, in a style of superior elegance. How many pages they [the committee] calculated upon is not known, but it must have required a mighty mind to extract matter sufficient to be worth eight dollars, from the history of three years of a child's life, even if that child were Zerah Colburn." Here we have a passage which may at first appear egotistic; yet the context is surely not inordinately vain. If instead of the words "Zerah Colburn" we read, "probably the greatest calculator on record," we have said the worst, and simply shown another passage in which Colburn's honestly mistaken opinion of himself comes to light. It is safe to say that but for a misinterpretation of passages of the first two classes, no one would attach any special importance to those of the third class as proofs of vanity.

Furthermore, Colburn is in several passages perfectly frank in stating his own defects. In fact, one of his critics, as extreme in one direction as Scripture and Binet are in another, speaks of the *Memoir* as "an inane production, which would be tedious in the extreme except for its absurd *naïveté* and the frankness with which the author admits his mediocrity." (*Spectator*, 1878, p. 1208.) The following passage from the *Memoir* (p. 104) will illustrate the basis on which this critic rested his estimate of the book:

"At the period of his entrance at Westminster school, he [Colburn] was a few days over twelve years old—quite old for the class in which he was placed, but for that reason better able, as well as by his eight months' attendance at the Lyceum in Paris, to get speedily removed into a higher class. During the two years and nine months that he was connected with this institution, he accomplished the labor for which the boys generally spent four or five years. He learned with facility, and the continual practice preserved what he acquired fresh in his memory. It is, however, a truth which may as well be stated here as anywhere else, that the mind of Zerah was never apparently endowed with such a talent for close thinking on intricate subjects as many possess. He was not peculiarly fortunate in arriving at a result which did not readily present itself, or for which the process leading thereto was not soon discovered. It is for this reason that he has been unable to discover a prospect of his extensive usefulness in mathematical studies, or of justifying the high expectations which many had reasonably formed on account of his early endowment, and hence he feels more reconciled than he otherwise might in abandoning the wisdom and literature of this world for the duties of his present important calling [the ministry]. While in school he generally sustained himself among the four at the head of the class; but was not remarkable either for quickness of mind or closeness of application."

Again, in trying to account for his gift, he says (pp. 165-6), "If the notoriety of his youth was designed [by his Maker] as an introduction to him in his ministerial capacity, it would be a natural expectation that his talents as a Preacher would be equal, if not superior, to the striking displays of his early precocity. This howev[er] is far from being the case."

One other possible criticism remains to be met: it may be said that Colburn's vanity is proved by the very fact of his writing an autobiography. But the plan of writing a memoir, as we have seen, did not originate with him; and it is only when (p. 165) "at length his situation has become such that an effort was necessary to obtain some pecuniary means for supporting a wife and three little girls, over and above the contributions of the people among whom he has been laboring during the past year," that "for want of any more promising employment, this has been undertaken."

The statement seems to be warranted by the passages quoted from the *Memoir*, and the facts presented as bearing on them, that Colburn was, on the whole, free from vanity, and erred only in accepting the uncritical popular estimate of himself. Even here his error was far less than we might have expected; in fact, the wonder is that a child who was so constantly before the public in his early years, so praised and marvelled at by famous mathematicians as well as by popular audiences, did *not* develop into just the sort of vain fool that Scripture and Binet have accused him of being. But if he ever had any illusions in this matter, the hard knocks of his later life effectively removed them. In the *Memoir* he stands before us as the painstaking and conscientious historian of "a very remarkable fact in the annals of the human mind"; and while he is not alway skillful, and at times becomes, it must be confessed, too much of a preacher to be an ideal historian, his book must be taken seriously, as an important contribution to the literature on mathematical prodigies.

APPENDIX II.

GENERAL TABLE.

The following table gives a bird's-eye view of the more important mathematical prodigies, for convenience in comparing the different men. In the column headed "heredity" are found such possibly relevant facts as are known about each man's parents and relatives. In the next column is a brief account of the circumstances attending the "development" of the calculating power; *precocity* here refers only to calculation, unless "all-round precocity" is specified. Under "education" are described both the general and the mathematical training of the calculator. The next column deals with the scope and methods of his mental calculations; the next, with his figure-memory (extent, etc.), and his probable memory type; a few facts about his memory in general are added in one or two cases, but usually this has not been considered necessary. In the last column are noted other peculiarities of the calculator, whether connected with his calculations or not.

It has not seemed worth while to extend the table to other prodigies, owing to the meagreness of the available data, and the sacrifice of compactness that would be involved. It is hoped, however, that the table here given will be found helpful. Every effort has been made to render it accurate, and as complete as was consistent with the desired degree of condensation.

NAME.	HEREDITY.	DEVELOPMENT.	EDUCATION.	MENTAL CALCULATION.	MEMORY.	REMARKS.
Tom Fuller (1710-1790)	Brought from Africa as a slave, at age of 14.	Probably not precocious; first records of his calculations find him at age of 70.	None.	Reduction of years etc., to seconds; sum of a geometrical progression; 9 figs. x 9 figs. Very slow calculator.	Probably of auditory type.	
Jedediah Buxton (1702-1772)	Father and grandfather men of some education.	Perhaps not precocious; but mental free beer record dates from age of 12.	None; very stupid from childhood; grasped even mathematical problems with difficulty. Practically no interests outside of calculation.	Handled immense numbers; once squared 39-fig. no. Methods clumsy, extremely slow; never got much beyond counting stage.	Kept mental free beer record. Probably of auditory type.	Could calculate areas pretty accurately by pacing. Could calculate while working, or carry on two different calculations a once.
Zerah Colburn (1804-1840)	Son of a farmer of little education. Supernumerary fingers and toes hereditary.	Began to calculate when 5 years old. Power developed gradually, and gradually deteriorated through lack of practice.	Fairly good, but intermittent. Showed moderate liking for mathematics, but preferred languages.	Ordinary arithmetical operations, multiplication by 5 figs., square and cube roots and factors by use of 2-figure endings.	Figure memory fairly good. Of auditory type.	Supernumerary fingers and toes. Nervous contortions in his early years when answering questions.
Henri Mondeux (1826-1862)	Son of a wood-cutter.	Tended sheep at age of 7; learned mental calculation by using pebbles. Became a professional calculator.	Received private instruction in mathematics, and showed considerable aptitude up to a certain point.	Ordinary arithmetical operations, involution, simple algebraic problems, partly by algebraic methods; ingenious processes.	Memory fair for figures, poor for other things. Probably of auditory type.	Could attend to other things while calculating.
Jacques Inaudi (b. 1867)	Family not talented; prenatal influence (?)	Began calculating at age of 6, while tending sheep; at 7 could multiply 5 figs. by 5 figs. mentally. Became professional calculator.	Very limited; learned to read and write at age of 20.	Subtraction with two 21-fig. nos., addition of five 6-fig. nos., 5 figs. x 5 figs., division of two 4-fig. nos.; simple algebraic problems by trial.	Auditory type; somewhat absent-minded; highly developed figure memory, but forgets unimportant figures after a short time.	Talking during his calculations slightly delays but does not confuse him.

NAME.	HEREDITY.	DEVELOPMENT.	EDUCATION.	MENTAL CALCULATION.	MEMORY.	REMARKS.
Ugo Zaneboni (b. 1867)	Mother had good memory.	Calculations began at age of 12; well developed at 14.	Fair.	Memory feats based on railway and similar statistics, evolution with aid of 2-fig. endings, also roots of imperfect powers, probably by trial and memory.	Perhaps of visual type; good figure memory; possibly has a simple number-form.	
Pericles Diamandi (b. 1868)	Mother had good memory; a brother and a sister share his gift for calculation.	Excelled in mathematics at school, aged 7-16; discovered calculating power on entering business, aged 16.	Good; excelled in mathematics; knows five languages.	Multiplicaton up to 5 figs. x 5 figs., etc. Uses cross-multiplication. Calculations slow.	Good memory; visual type; has a number-form, and colored audition for some names.	
Johann Martin Zacharias Dase (1824-1861)		Attended school at age of $2\frac{1}{2}$, took to the stage as a calculator at 15; probably precocious in calculation.	Slight; stupid in everything but calculation, even including mathematics.	Practically unlimited power of handling large nos.; 100 figs. x 100 figs. in $8\frac{3}{4}$ hours. $\sqrt{100 \text{ figs.}}$ in 52 min. Computed logarithm and factor tables, etc.	Prodigious figure memory. Perhaps of visual type.	Could count some thirty objects at a glance.
George Parker Bidder (1806-1878)	Son of mason; one brother had a remarkable memory for Bible texts, another was a good mathematician; a nephew had great mechanical talent; son was excellent mathematician and mental calculator; two granddaughters were above average ability in mental arithmetic.	Learned to count at age of 6, and soon became excellent mental calculator; retained the power through life, using it in his profession. Used pebbles, shot, etc., at first.	Good; was a man of wide interests and of considerable ability, both mathematical and general.	Ordinary arithmetical operations, multiplication up to 12 figs. x 12 figs., compound interest. Roots and factors by the aid of 2-fig. endings, etc. Methods often original and highly ingenious. Calculated rapidly.	Of auditory type in his calculations, but had good visual memory for diagrams, etc.	

NAME.	HEREDITY.	DEVELOPMENT.	EDUCATION.	MENTAL CALCULATION.	MEMORY.	REMARKS.
GEORGE P. BIDDER, JR., Q. C. (the younger Bidder) (b. 1837)	Son of G. P. Bidder.		Good; was 7th wrangler in 1858. Practiced law.	Could multiply 15 figs. x 15 figs., but slowly, and with occasional errors. Used cross-multiplication.	Visual type; has number-form. Uses a system of mnemonics in his calculations.	Possibly an "artificial" calculator, in imitation of his father.
TRUMAN HENRY SAFFORD (1836-1901)	Father interested in mathematics, mother of nervous temperament; both had taught school.	All-round precocity; began to calculate between ages of 3 and 5; development steady; studied higher mathematics at 8, computed and published almanacs at 9 and 10.	Good; interest in all studies, but especially in mathematics and astronomy.	Ordinary arithmetical operations; multiplication of two very easy 18-fig. nos. Roots and factors by aid of 2-fig. endings. Rapid calculator.	Memory encyclopedic in scope; of auditory type.	Nervous contortions, or at least great restlessness, during calculations in boyhood.
ANDRÉ MARIE AMPÈRE (1775-1836)		All-round precocity; counted with pebbles, etc., at age of 3 or 4.	Good; all-round scholar.	Specific information lacking.	Perhaps of auditory type. Very retentive general memory.	
CARL FRIEDRICH GAUSS (1777-1855)	Maternal uncle mechanically and mathematically gifted.	Precocious in several directions; began mental calculation in his third year, and probably retained the power through life.	Good all-round education; became a mathematician of the highest rank.	Specific information lacking; probably, like Safford, an all-round mental calculator, by natural and book methods combined. Made use of logarithms in his mental calculations.	Very tenacious figure-memory; perhaps of auditory type.	

PSYCHOLOGICAL LITERATURE.

The Psychology of Reading. An Experimental Study of the Reading Pauses and Movements of the Eye. WALTER FENNO DEARBORN, Ph. D. Archives of Philosophy, Psychology, and Scientific Methods, No. 4, March, 1906. The Science Press, New York, 1906. pp. 132.

Dr. Dearborn's study marks a material advance upon the work done thus far upon the movements and pauses of the eye in reading. His primary object was "to determine with exactness the form and character of the movement of the eye in reading, and to define or plot the positions on the page which correspond to the so-called fixation or reading pauses of the eye."

A modification of the Dodge falling plate camera was used, giving "continuous photographs of the horizontal movements of the corneal reflection made upon a slowly falling photographic plate of great sensitiveness." The time of the movement is marked on the plate by the alternate admission and exclusion of light by a spring pendulum oscillating before the plate.

The place of the fixation pauses was determined, for the most part, by projecting the original photographic record upon a stereopticon screen, magnifying and adjusting this record to coincide with a drawing on the screen representing the length of the lines of print. A drawing of the record could then be made which could be placed "over the lines of the proper pages." The marking of the fixation pauses "is accurate to the limits of a single small letter of a newspaper page," save for certain important sources of error noted by the author as due to movements of the head and to the semi-nystagmatic movements of the eyes. Eight adults and three children were tested.

The rate of movement was found to vary considerably, but on the whole the measurements agree with those made by Dodge, and the rate is too fast for perception during the movement. However, some readers made certain slow shifting movements, sometimes ten to twenty times slower than the usual movement, accompanying the movements of the attention. These shiftings are classed as fixations, since they furnish data for perception.

It was found that the eye readily falls into a brief "motor habit" of making a certain fixed number or succession of pauses per line, for a given passage, and independently of the nature of the subject matter. Fast readers do this especially, and most readily in the shorter lines of uniform length. The first pause in each line is the longest one, the eye seeming to take a general summary at first, and there is a secondary long pause near the end of the line. At these pauses the attention is thought to expand and a more general perception is secured.

The exact point fixated may be in any part of the words, or in the spacing between them. It apparently "pays little attention to many of the laws of apperception or the rules of the rhetorician." These exact points of fixation are "significant only as representing the point about which are grouped the 'block' of letters that are simultaneously perceived as one word or phrase complex. It more often falls in the first third than at the centre of a given perception area."

More pauses are made with relative pronouns, prepositions, conjunctions, auxiliary verbs, and numerals, than with substantives, adjectives, etc. The pauses are longer and more numerous in reading slowly. Readers when asked to read as fast as possible saved nearly one-third of the time usually taken, and the number of pauses was reduced one-third. Eye fatigue was found to increase the number and duration of the fixation pauses.

Very great differences were found in the rate of reading when the subjects were given matter of special interest to each, and there was the same ranking in rate, with smaller differences, for *all* classes of reading matter. Rate differences between individuals and in the same individual are thought to depend largely, when other conditions are constant in "the ease with which a regular rhythmical movement can be established and sustained," consisting of a regular number of pauses per line and a fairly uniform arrangement in the order of long and short pauses. Lines of moderate and practically uniform length fulfil these conditions best.

Children were not found to have a different rate of eye movement from adults, but their fixation pauses are more frequent and generally longer, though quite short ones occur. Their "accuracy of fixation appears as exact as that of the adults."

Dr. Dearborn's study gives evidence of care and thoroughness on the main points studied, and presents a number of minor observations and interpretations which will be of value to those who study the subject further. EDMUND B. HUEY.

L'Attention spontanée dans la vie ordinaire, et ses applications pratiques, by ROERICH. Revue philosophique, No. 8, Août, 1906. Trente et unième année. pp. 136-156.

An element of volition characterizes all attention, yet spontaneous and voluntary attention are to be definitely distinguished. Spontaneous attention is discussed under two aspects,—primitive and apperceptive.

Primitive attention marks essentially the capacity of the child, or of the uncultivated man, yet it is a most important factor in the life of the scholar, or the artist. It is restricted to the interpretations of such particulars of the external world as are accessible to the senses, and is absolutely necessary in putting the mind in possession of elementary facts preparatory to classification and generalization. Molière, Rousseau and Zola excelled in this power, while in Voltaire and Racine it was markedly lacking.

The laws of primitive attention are as follows:

1. According as the individual is more or less attentive, the time of reaction after perception is shortened or lengthened.
2. However vivid the stimulus, primitive attention cannot fix an object for more than a few seconds at a time.
3. In every change from one perception to another, time must be allowed for the exercise of judgment upon this change.

The psycho-pedagogical rules based upon these laws are:

1. To hold the attention, impressions must move progressively in intensity or vividness. (This progression soon reaches a maximum.)
2. After each impression, a delay for recovery (neither too long, nor too short) must be allowed.
3. To waken primitive attention, the presentation must be nicely determined.
4. Impressions of a different nature, when they relate to the same object, may be allowed.
5. Primitive attention is the most certainly aroused by contrast among successive or simultaneous impressions.

In apperceptive attention, the new impression is reinforced by mental contents similar to itself. It is not really indispensable that this reinforcing mass be logically co-ordinated, or scientifically classified; yet, to secure the best results, the apperception should be carefully prepared. Rabelais and Rousseau were masters in this kind of preparatory work. The failure of Pompey and Cicero may be traced to the want of this power to hold the attention of the public. Julius Cæsar was pre-eminently successful in this direction. To skill in preparing for apperceptive attention, is due the triumph of realism over idealism.

The fundamental law of apperceptive attention is:

In every apprehension which is not directed by the will, the accuracy and rapidity of understanding increase in proportion to the extent, the variety, and the judicious co-ordination of the associations of the acquired ideas. Upon this are based the following rules:

1. In apperceptive attention, it is not indispensable that the presentation be new, but it must seem to be new. Racine, Glück, Goethe, Chateaubriand, had the power of presenting well-known legends in such a way that the ideas possessed all the attraction of original creations.

2. To facilitate apperceptive attention, the content of the new must be similar to the old experience, but, by no means identical with it. (The tediousness of copying illustrates this point.)

3. New notions must be connected with old experience by means of transitional notions which shade gradually from the old into the new content. (The Odyssey of Homer, the Dialogues of Plato, the tragedies of Sophocles, illustrate admirably the significance of such transitions as a means to holding the attention.)

4. Between two culminating points of apperceptive attention a pause should be made. This pause cannot be regarded as a definite cessation, but implies rather a kind of slackening of activity, by means of which, a more perfect assimilation through talent or unconscious cerebration may be secured. Without this pause after a culminating point, the attention may not be secured for the second point. This is explained on the ground of the fatiguing nature of apperceptive attention. Apperception is always a source of emotion more or less, and while it constantly satisfies, it constantly gives rise to new expectation, which implies a constant outlay, and a consequent exhaustion of energy, the renewal of which demands at least an approximate rest after a period of mental activity.

MARGARET KEIVER SMITH.

Mental and Moral Heredity in Royalty. A Statistical Study in History and Psychology, by FREDERICK A. WOODS. New York, Henry Holt & Co., 1906. pp. VI, 312.

Dr. Woods' book is an amplification of a series of articles published during the last few years in the "Popular Science Monthly." The preface states that it was written in the hope of contributing something to a knowledge of the science of history, but it is, however, rather a contribution to the psychology or the comparative psychiatry of history. The analyses of the various royal family trees are minute and painstaking, but it seems unfortunate that the author should let his biological enthusiasm hide the sound principles of modern psychiatry. Because the family trees of royal houses are so minutely recorded, they offer a splendid opportunity for the study of the question of mental heredity, and yet it appears to us that the author has not made the best use of available historical facts. Because certain members of a family suffered from a vague disorder known as insanity, and certain descendants or collateral individuals of this family suffered from a like disorder, is no proof of the hereditary basis of the mental disease, unless we are able to state the exact nature and cause of the malady. Terms like insanity, psychosis, or imbecility found affixed to the names of various royal personages throughout the book, are mere hints that the Nemesis of heredity is at work grinding slowly but surely. These terms fail to furnish the more exact information of what particular type of mental disease was transmitted. Periodic depressions and exaltations (manic-depressive insanity), the deterioration associated with the adult onset of chorea (Huntington's chorea), certain types of adolescent insanity (dementia præcox), moral deficiencies, alcoholism, or epilepsy, are particularly prone to run in certain families and appear as like or closely allied disorders in the offspring. It is only under these conditions that we can speak of an hereditary stigma. But if an ancestor became insane from trauma, or was the victim of a febrile delirium or of some poison, accidentally ingested, and if a mental disease of a different or even of a like accidental type appeared in the offspring or collateral branches, it would be transcending all facts to speak of heredity under these conditions. The classification of intellect and morals into ten grades, while admirable for the purpose of comparative study, is rather artificial and does not possess that flexibility which is so characteristic of individual moral and mental tendencies. While the book lacks in the few details pointed out above, yet otherwise it is admirable as a study of the psychology of history and of the relentless grasp of general hereditary factors on certain families. The use of Galton's law of ancestral heredity and of modern biometrical methods is a novel one, the results are admirable and worthy of a wider application. The book takes up in detail the different houses and branches of the principal European countries, while the bibliography is unusually complete and offers a good working basis for all future investigators along these lines. The illustratus are admirable, especially those showing the Hapsburg lip. The various tables and pedigrees of the different royal families show a wide grasp of the subject and enable one to tell at a glance the mental and moral attributes of the various royal personages and their relation to one another in the same group. I. H. CORIAT.

L'Année psychologique. Publiée par A. BINET. Douzième année, 1906. Paris, Masson et Cie. pp. 672.

This volume of the *Année psychologique* divides, in the usual way, into original articles, general reviews of the past year, and critical notices of psychological works.

The original articles, although not all equally satisfactory, are of quite exceptional range and interest, and the editor is to be heartily congratulated upon the variety and quality of the work which he has brought together. The series opens with a paper, by MM. Binet and Simon, on physiological and social poverty, in which the authors (taking advantage of a recent proposal to issue medical report-cards for school children) recommend certain methods of testing children for backwardness, etc., with a view to the juster distribution of state aid. In the following article, M. Bonnier discusses the mental status of the bees, and decides for intelligence as against reflex action. M. Treves contributes an elaborate study of work, fatigue, and effort, in the course of which he describes new instruments and methods, the results of which lead to valuable and definite results. M. de Sanctis discusses the types and degrees of mental defect, and indicates a method of test. M. Bourdon presents an experimental investigation of the influence of centrifugal force upon the perception of the vertical. M. Blaringhem writes upon the idea of species and the theory of mutation as set forth by de Vries. M. Binet enters a plea for a philosophy of mind, in the course of which he scores unmercifully Haeckel's recent *Riddles of the Universe*. M. Bohn criticises the terms tropism, reflex, and intelligence, and seeks to show that the work of Jennings is the complement of that of Loeb. M. Larguier des Bancels contributes an important and very readable paper on the psychology of evidence. M. Binet enters upon certain questions of scientific pedagogy, discussing in collaboration with M. Simon the measurement of visual and auditory acuity, and in collaboration with M. Vaney various tests of intelligence and the correct attitude to be adopted in writing. M. Claparède supplies a critical article upon the psychology of the court-room. Finally, Professor Mach gives a succinct account of his views upon the relation of physics to psychology.

There follow general reviews of a number of fields more or less directly related to psychology: anatomy of the nervous system (Van Gehuchten), general physiology of nerve cells and fibres (Friedericq), sensation (Nuel), pedagogy (Chabot), æsthetics (Souriau), linguistics (Meillet), comparative psychology (Bohn), criminological statistics (Lacassagne and Martin), anthropology (Deniker), abnormal psychology (Decroly), metapsychics (!) (Maxwell), religious psychology (Leuba), philosophy (Malapert), mental pathology (Leroy), pathology of the nervous system (Guillain). The consequence of this extended programme is that very little space is left for critical notices of psychological books and articles; so that we have the somewhat paradoxical result of an *Année psychologique* with the central and, perhaps, the most important aspects of psychology crowded out. The editor, however, hopes to remedy this defect in future numbers by bringing all the principal divisions of psychology within the domain of the general review. P. E. WINTER.

Outlines of the Evolution of Weights and Measures and the Metric System. By W. HALLOCK and H. T. WADE. New York. The Macmillan Co., 1906. pp. xi, 304.

The authors of this work propose "to consider briefly and systematically the general history of weights and measures, the scientific methods by which units and standards have been determined, the concrete standards by which the units are represented, and the present aspect of modern systems of weights and measures, together with the difficulties and advantages involved in any proposed changes." They have accordingly aimed to supply, first, an introduction to metrological science designed especially for the student entering on the study of physics; and, secondly, such a discussion of the present status as may lead to an intelligent understanding of the issues involved in the proposed adoption of the metric system by English-speaking peoples. The ten chapters into which the work is divided deal with the beginnings and development of the science of metrology; the original development of the metric system; the extension of this system throughout Europe and elsewhere; the weights and measures of the United States; the characteristics and principles of the modern metric system; its use in commerce, in manufacturing and engineering, and in medicine and pharmacy; the international electrical units; standards and comparison. An appendix gives tables of equivalents and useful constants.

The authors have thus compiled an interesting, and, so far as the layman may judge, an accurate and useful book. Themselves supporters of the metric propaganda, they have not overloaded thei pages with controversial matter, but preserve throughout a tone o scientific impartiality. Numerous references to original sources ena

ble the reader who so desires to prosecute his studies further for himself.
P. E. WINTER.

Essays. Von WILHELM WUNDT. Zweite Auflage, mit Zusätzen und Anmerkungen. Leipzig, W. Engelmann, 1906. pp. vi, 440.

The *Essays* of 1885 have long been out of print, and many psychologists must have entertained the hope that their author would some day bring out a new edition of them; an edition enriched, perhaps by the addition of various important articles from the *Philosophische Studien*. The new edition has now appeared, and in a guise which makes the book one of extraordinary interest. Professor Wundt has reprinted the original essays (with the exception of the two on Animal Psychology and on Feeling and Idea) practically without change, and has appended to each essay a postscript—sometimes consisting of a couple of historical or autobiographical paragraphs, sometimes amounting to a new and separate treatment of the subject—expressing his present views upon the topics discussed. We thus have before us, in the words of the preface, "zwei Epochen wissenschaftlichen Denkens in zwei zeitlich getrennten Bildern einander gegenübergestellt." There is, probably, no single volume which better shows the development of psychology during the past twenty years, or which brings home more forcibly to the reader the range and depth of Wundt's influence upon that development.
P. E. WINTER.

The German Universities and University Study. By F. PAULSEN. Authorized translation by F. Thilly and W. W. Elwang. New York, Chas. Scribner's Sons, 1906. pp. xvi, 451.

Professor Paulsen's book on German Universities is, as Professor Thilly puts it in his introduction, "the most satisfactory exposition of university problems and the most helpful practical guide in solving them, that has been published in recent years." And the present translation will, no doubt, satisfactorily replace Hart's "German Universities" for the English-speaking reader; that work, excellent as it is, dates from 1874 and is consequently quite out of date. Professor Paulsen first gives an outline of the historical development of the German university, and discusses the modern organization of the universities and their place in public life; then proceeds to discuss in detail the function of the university teacher and the ideals of university teaching; passes from teacher to student, and from instruction to study; and closes with an account of the particular university faculties. The volume ends with a bibliography, a list of the German universities, and an index of names and subjects.

The translation is for the most part acceptable, if it is by no means brilliant. "Talmi-elegance" is not a word that one would care to see incorporated in the language; and "it would not be dignified to write for such" is not a sentence that one can qualify as even talmi-elegant.
H. E. HOTCHKISS.

A New Interpretation of Herbart's Psychology and Educational Theory through the Philosophy of Leibniz, by JOHN DAVIDSON. William Blackwood and Sons, Edinburgh and London, 1906, pp. 191.

This treatise is, with a few modifications, a thesis accepted by the Senatus of Edinburgh University in 1905 for the degree of Doctor of Philosophy and is, to quote the words of the author, "an attempt to give a general, and, it is believed, a new interpretation of Herbart's psychological and educational theories so as to show the adequacy of his fundamental conceptions to meet at least some of the demands of a science of education. In particular, there is an attempt to show first, that Herbart's psychological standpoint is the only intelligible and workable standpoint for the practical teacher; and, second, that from this standpoint such definite connotations can be given to the terms soul or mind, knowing, feeling, will, interest, and habit, that the terms so connoted become scientific and guiding concepts for educational practice."

The author first discusses some of the critics of Herbart, dissenting from those who, from the Kantian point of view, regard the theoretical foundations of Herbart's pedagogy as thoroughly unstable, and from the position taken by Dr. Hayward and other Herbartians, who, while admitting the incompleteness or even erroneousness of Herbart's metaphysics and psychology, maintain that this in no way militates against the practical value of his educational doctrines, which were not deduced from his philosophy. Dr. Davidson, on the contrary, maintains that if the Herbartian theory of education works in practice, it implies a sound psychological theory and that the logical procedure is to question whether Herbart has been rightly interpreted, rather than to assume a contradiction between theory and fact. He finds the key to Herbart in the philosophy of Leibniz and through this offers a new interpretation of Herbart. In order to make the relation clear, he devotes three chapters to the Leibnizian philosophy, discussing successively Leibniz's philosophical principles, his psychological standpoint, and his theory of feeling and will. He then proceeds to discuss Herbart's psychological standpoint in comparison with that of Leibniz, beginning with his definition of soul and taking up point by point the similarities and differences of the two systems and showing that the latter are more in seeming than in reality. He then discusses in a similar way, Herbart's theory of presentation, theory of feeling, theory of will, and concept of interest, summarizing the main points of his argument as follows: "Soul life is life in and through presentations and knowledge. Will is the movement of presentations or knowledge, and meaningless when regarded as separable from knowledge. Hence right knowledge *in movement* will imply right willing. But the soul life can be habituated to move in right presentations or knowledge by the educative instruction of the educator, which secures that the right presentations are sufficiently often repeated in the soul-life to become habituated soul activities. The conception of the 'Memory of Will' is adopted by Herbart to account for the growth of this habituated soul activity. The various habituated activities ultimately form the soul-life into an organized instrument—an organon called interest—which wills, in the truest and highest sense of willing, the moral life of thought and action." Then follow chapters on the fallacy of formal education, individuality, and many sided interest, and interest versus self-realization as the first principle of education. The book is throughout clearly and logically written and is a valuable contribution to the philosophy of education.

THEODATE L. SMITH.

The Origin of Life, by J. BUTLER BURKE. Chapman & Hall, London, 1906. pp. 351.

In Nature, May 25th, 1905, Mr. Burke published a short account of experiments carried on by him in the Cavendish Labaratory at Cambridge, which evoked great interest and much discussion. A somewhat more elaborate account appeared in the September number of the Fortnightly Review. These experiments dealt with certain forms of radio-activity, and in the course of them Mr. Burke discovered certain minute bodies, which he named radiobes, and which he claimed exhibited certain characteristics of living matter. He characterizes them as follows: Radiobes are "neither crystalline nor colloids in disguise, though colloids in the aggregate, but something more, and crystals in their constituent parts." They differ from both in "possessing the elements of vitality in a primitive and undeveloped state." The widespread interest in these experiments, now elaborated in book form, was due to the fact that they dealt with the question of spontaneous generation in a new form. Not that Mr. Burke is the only investigator at work on this problem, as Raphael Dubois, M. Benedict and Renaadet have all published researches along this line, but Mr. Burke's book is the first extensive English publication dealing with this phase of the subject whose problem is not of known organisms, but of the character of hitherto unknown bodies which some experimenters, among whom is Mr. Burke, are inclined to consider as living, while others regard them merely as peculiar crystals, having possibly traces of organic impurities mingled with them. The production and behavior of Mr. Burke's radiobes is, briefly stated, as follows: When sodium bromide is allowed to drop upon beef gelatine contained in a sterilized tube, by means of a small inner tube which can be broken without opening the test tube, after an interval of about twenty-four hours, minute bodies appear. These bodies, according to Mr. Burke, develop a nucleus, grow, and after reaching a certain size, subdivide. This subdivision constitutes their strongest claim to life and is denominated by Mr. Burke as "reproduction of a degenerative sort," since the cycle is not continued when subcultures are made. But strangely enough, in view of its fundamental importance for Mr. Burke's theory, he nowhere hints at having subjected these bodies to continuous observation which, however tedious and wearisome, would be a necessity for scientific thoroughness and accuracy, nor does he claim even to have seen the actual process of division. The statement that they do divide is apparently an inference from successive appearances under the microscope, but how easily juxtaposition may be mistaken for fission is a matter well known to microscopists. An excellent example of these appearances may be found in Rainey's figures, reproduced in Bastian's Beginnings of Life. But admitting that fission did occur, it is not shown in what specific ways it differs from the fission observed in experiments with fatty matters derived from dead animal substance. Moreover, radiobes are soluble in warm water, a fact difficult to reconcile with any generally accepted theory of conditions under which life first appeared. A. L. Herrera, in the *Revue Scientifique* of Feb. 17, 1906, thus expresses his opinion as to the nature of these bodies. "As far as Burke's radiobes are concerned, they would appear to be simply accidental crystals of carbonates of barium and calcium, swelling in organic liquids or media. . . . These crystals, which have been studied conscientiously by Harting and Rainey, are attackable by weak acids, which have an albuminoid or fatty nucleus."

As a whole Mr. Burke's book bears the marks of having been hastily thrown together. The few references are so incomplete and inaccurate as to be practically worthless, and important theories and experiments are quoted with no clue as to their date or place of publication. The chapters in which Mr. Burke records his own experiments, where, if nowhere else, the reader has a right to expect accuracy and clearness of statement, leave one with a good deal of uncertainty as to the methods of observation and actual progress of the experiments, and an error in the numbering of the plates makes the references to the various appearances which they are supposed to illustrate, unintelligible. It is to be regretted that the publication of this book was not deferred until it could be completed in a more accurate and scientific form.

THEODATE L. SMITH.

The Unit of Strife, by E. K. GARROD. Longmans, Green & Co., New York, 1905. pp. 194.

In this interesting volume the author shows how the struggle for life among individuals has given place to struggle among larger and larger units, while co-operation has taken the place of struggle among the individuals making up the larger unit. Nations are on the old basis of the struggle for existence; but the individuals forming a nation must co-operate if the larger unit is to be strong. This difference in the basis of activity for states and individuals gives rise to a different code of morals for the two.

The larger part of the book is devoted to tracing the transformation of man from competitive individualism to sympathetic co-operation. The great instruments of this transformation have been religion and law. These instruments have not overcome the force of selection entirely, but the process of socialization has been hastened as individuals have come to comprehend the laws of social solution. Those nations will be most likely to survive that have religious systems most conducive to co-operation. The religious idea must be a progressive one.

The book is suggestive, but it is somewhat one-sided in its treatment. Struggle between individuals, which is still of immense importance in progress, is relegated to the background, and religion is treated merely as a socializing force and is not recognized as a means of individual development. F. A. BUSHEE.

BOOK NOTES.

Die Hoffnungslosigkeit aller Psychologie, von P. J. MÖBIUS. Carl Marhold, Halle, 1907. pp. 69.

This is a most noteworthy publication. The author has studied thoroughly the systems of the theory of knowledge in its various aspects and at the end, like Faust, finds himself convicted of ignorance and penetrated with a sense of hopelessness. This is an almost inevitable logical result. Indeed, we have been waiting for years for some one to draw this inference. Once grant the assumption that the soul is best informed about its own processes and that the external world, as well as our personality, is but a universe and there is no issue, save in pessimism and depair, for consciousness lives in a charmed circle which it can never transcend. To our thinking, however, the issue is not despair, but the only too long delayed collapse of this method makes all the more plain and mandatory the necessity of a method which frankly assigns priority to the things of sense. But this is a long story.

Modifiability in Behavior. Factors determining direction and character of movement in the earthworm, by H. S. JENNINGS. Reprinted from the Journal of Experimental Zoölogy. Volume III, No. 3. Baltimore, 1906. pp. 435-455.

Professor Jennings has been at work these many years in studying the behavior of lower unicellular organisms and has here brought together in a more comprehensive way than in his Carnegie publication the results of his work on amœba, bacteria, paramecia and other infusoria. He has also done much work on the lower metazoa and here analyzes their reactions to light, food, etc. Two most interesting chapters are upon the development of behavior and its psychic relations in lower organisms. No one has done such careful and thorough work so near the beginnings of genetic psychology.

Studies in Philosophy and Psychology, by former students of Charles E. Garman, in commemoration of twenty-five years of service as teacher of philosophy in Amherst College. Houghton, Mifflin & Co., Boston, 1906. pp. 411.

Thirteen original contributions by his former students, including such names as Professors Tufts, of Chicago; W. F. Wilcox, of Cornell; F. C. Sharpe, of Wisconsin; F. J. E. Woodbridge, of Columbia; E. L. Norton, of Western Reserve Univ.; W. L. Raub, of Knox College; E. W. Lyman, of Bangor Theological Seminary; E. Delebarre, of Brown; E. J. Swift, of Washington University; A. H. Pierce, of Smith; R. H. Woodworth, of Columbia, are here printed in their dedicatory volume commemorative of Professor Garman's twenty-five years as a teacher of philosophy at Amherst College. In lieu of preface, a letter addressed to Dr. G. Stanley Hall, several years ago and printed in the *American Journal of Psychology*, is here reproduced, as it is Professor Garman's best explanation of his system and his theory of teaching. Any academic professor would be proud of such pupils and of such a token of their respect. Professor Garman's work has been quite unique, as he has refused to be seduced into extreme positions of the epistemologists and has wrought out a system of philosophy, which while often criticised for being esoteric, seems to be admirably adapted to deal with the callow doubt of college students in a way that does not force upon them premature conclusions and which stimulates those who have the ability to go on and become experts, and which, at the same time, lays a firm ethical foundation for contact with life. Precisely what this system is, Professor Garman's colleagues are curious to know and many have criticised his methods of making all his instruction so personal and confidential. Probably, like all vital growing minds, Dr. Garman is constantly recasting and improving and those who wait will in the end be gainers by the delay. He certainly avoids more successfully than any other vigorous and original mind, any danger of immature expression by printing his system prematurely.

The Syllogistic Philosophy or Prolegomena to Science, by FRANCIS E. ABBOT. Little, Brown & Co., Boston, 1906. Vol. I, pp. 377. Vol. II, pp. 376.

We welcome heartily these two stately and well gotten up volumes as an original contribution by one of the most acute minds, in this field, that this country has ever produced. The work was left incomplete at the tragic death of its author, whose wife, with commendable piety, has finished and brought it out. Dr. F. E. Abbot began life as editor of the Index, a journal of free religion, and leader of the liberal religious movement in this country. Both the man and the journal were gratefully received and did a great deal to clear up the ultra orthodox atmosphere of New England and of the country at large. He later abandoned this work and came over to philosophy for which he had remarkable aptitude. No one that the writer of this note has ever met has been endowed with greater power or more subtlety to trace the ultimate end of the very many devious ways of philosophical thought.

The Hygiene of the Mind, by T. S. CLOUSTON, M. D. Methuen & Co., London, 1906. pp. 284.

Dr. Clouston is now the head of the most important institution for the care of insane in Scotland, and, in view of the fact of the volume of the publications of his pupils, may almost be called a founder of the current Scotch School. He and a group of younger men about him are not distracted by the problem now so extended in this country, whether the basis of scientific study here is neurological or systematic, but they seek to look at the whole individuality and not to lose sight of brain mechanism, heredity, temperament, social instincts, emotion, bodily diseases, hygiene and manners, blood, work and fatigue, subjects which constitute the headings of the chapters of this book. Two periods of childhood, one from birth to seven, and the other between the age of seven and fifteen, and the ten years of adolescence are discussed. Sex questions and stimulus are also included in the broad view of the subject taken by Dr. Clouston.

Harvard Psychological Studies. Vol. 2. Edited by HUGO MUNSTERBERG. Houghton, Mifflin & Co., Riverside Press, Cambridge, 1906. pp. 644.

We have here the second volume of these important studies, which comprise something over a score of papers classified as optical studies, feeling, association, apperception, attention, motor impulses, and animal psychology. Such a number of papers are naturally of very unequal value, but among the multiplicity of such studies it is something to say that most of these really seem to make contributions.

Instituts Solvay. Travaux de l'Institut de Sociologie. Notes and Memoirs. Bruxelles et Leipzig, 1906.

These works, admirably printed, represent the work of the Solvay Institute, which may be called, perhaps, the Carnegie Institute of Belgium. The importance and extent of these publications limits our giving any adequate description of their contents. Suffice it to say that they stand for the best collection of scientific work which the country, already distinguished for its savants, can produce. They will be indispensable to express knowledge of the departments represented.

Brain and Personality; or the Physical Relations of the Brain to the Mind, by WILLIAM HANNA THOMSON. Dodd, Mead & Co., New York, 1906. pp. 320.

This eminent practitioner has here attempted to bring together the results of his own rich experience on the relations between brain and soul. While the author is no doubt well read, the work gives little trace of erudition and is encumbered by almost no notes or references. After an historical introduction and a brief account of the physical basis of mind, he discusses brain weight, its significance as a double organ, speech, the evolution of the nervous system, sleep, and makes various practical applications.

The Nervous System of Vertebrates, by J. B. JOHNSTON. P. Blakiston, Son & Co., Philadelphia, 1906. pp. 370.

The writer here sets forth with admirable lucidity and with the aid of 180 diagrams the present status of our knowledge of the nervous system, beginning with its development, elements, functions, the efferent system, including the senses, the visceral and sympathetic centres, those of correlation, etc. It is a work that should be in the hands of every student, for it is just now the most concise summary of the recent results of science without being very much burdened with technicalities.

Proceedings of the Society for Psychical Research. Vol. 20, Part 3, October, 1906. Robert MacLehose & Co., Ltd., Glasgow, 1906. pp. 432.

Die Morphologie der Missbildungen des Menschen und der Tiere, von ERNEST SCHWALBE. I. Tiel. Allgemeine Missbildungslehre. (Teratologie.) Gustav Fischer, Jena, 1906. pp. 230.

Here we have in rather condensed form a very comprehensive view of the theory concerning monsters and deformities. The chapters are written with characteristic German exhaustiveness and comprise the history of the idea, the literature, relation to related sciences, the contribution of experimental influences upon development history, regeneration, comparative anatomy and heredity, physiology, the time at which teratological influences are most effective and common, their cause, the effects of certain mal-formations of the germ, of tumors, amuiogene, the relations to sex, and finally the clinical treatment. Many of these topics are illustrated by cuts often from specimens of the author. One of the most interesting of all the original chapters is a pedigree of eight generations of "bleeders" of a family originally called Mampel, and in the last generation having some eighty-five descendants. The father was a bleeder and two of his eleven children died of this disorder. It would seem that only boys are bleeders and that they never transmit this tendency. Girls are never bleeders but do transmit. If this all is true, it is of the utmost importance for the doctrine of heredity.

Haida Texts and Myths. Skidegate Dialect. Recorded by John R. Swanton. Smithsonian Institution, Bureau of American Ethnology, Bulletin 29. Government Printing Office, Washington, 1905. pp. 448.

The texts and myths here printed were obtained on the Queen Charlotte Islands of British Columbia during the year of 1900-01. About 85 pages are devoted to text itself, on the right-hand page, and the translation on the left. The remainder of the book is devoted to English translation of tales.

A Second Life, by S. TOLVER PRESTON. For private circulation. 1903. pp. 31.

Brains and Brain Preservatives, by ALES HRDLICKA. Proceedings of the United States National Museum. Vol. XXX, pp. 245-320. Goverment Printing Office, Washington, 1906.

Normale und anomale Farbensysteme, von A. KIRSCHMANN. Archiv für die gesamte Psychologie. VI, Bd., 4 Heft, pp. 397-424. Wilhelm Engelmann, Leipzig, 1906.

Das Wesen des menschlichen Seelen- und Geisteslebens, von BERTHOLD KERN. August Hirschwald, Berlin, 1905. pp. 130.

Philolaus, by WM. ROMAINE NEWBOLD. Archiv für Geschichte der Philosophie, 1905, Vol. 19, pp. 176-217. Georg Reimer, Berlin.

Le Mensonge. Etude de Psychosociologie Pathologique et Normale. Par G. L. DUPRAT. Félix Alcan, Paris, 1093. pp. 190.

1. Note sur desformules d'intro duction al'énergétique, physio- et psycho-sociologique, par E. SOLVAY. Bruxelles et Leipzig, 1906. pp. 26.

2. Esquisse d'une Sociologie, par E. WAXWEILER. Bruxelles et Leipzig, 1906. pp. 306.

3. *Les origines naturelles de la propriété; Essai de sociologie comparée*, par R. PETRUCCI. Bruxelles et Leipzig, 1905. pp. 246.

4. *Sur quelques erreurs de méthode dans l'étude de l'homme primitif: Notes critiques* par L. WODON. Bruxelles et Leipzig, 1906. pp. 37.

5. *L'Aryan et l'anthroposociologie: Étude critique* par le DR. E. HOUZÉ. Bruxelles et Leipzig, 1906. pp. 117.

6. *Mesure des capacites intellectuelle et energétique* par CH. HENRY, avec remarque additionelle par E. WAXWEILER. Bruxelles et Leipzig, 1906. pp. 75.
7. *Origine Polyphyletique, homotypie, et non-comparabilité des sociétés animales* par R. PETRUCCI. Bruxelles et Leipzig, 1906. pp. 126.

Leib und Seele, Darstellung und Kritik der neueren Theorien des Verhältnisses zwischen physischem und psychischem Dasein, von DR. RUDOLF EISLER. Johann Ambrosius Barth, Leipzig, 1906. pp. 217.

Outline of the Vedanta System of Philosophy according to Shankara, by PAUL DEUSSEN. Translated by J. H. Woods and C. B. Runkle. The Grafton Press, New York, 1906. pp. 45.

Space and Geometry, in the light of physiological, psychological and physical inquiry, by DR. ERNEST MACH. Translated from the German by Thomas J. McCormack. The Open Court Publishing Co., Chicago, 1906. pp. 148.

Contributo alla Dottrina delle Afasie, pel PROF. L. BIANCHI. R. Tipografia Francesco Giannini & Figli, Napoli, 1906. pp. 32.

Psychic and Economic Results of Man's Physical Uprightness. T. W. HEINEMAN. Pasadena, Calif., 1906. pp. 103.

Die Welt als Widerspruch. G. F. KROMPHARDT. Niagara Falls, N. Y., 1906. pp. 23.

Cesare Lombroso e La Filosofia Scientifico. PROF. ENRICO MORSELLI. Torino, 1906. pp. 354-384.

L'alcool e le malattie del sistema nervoso, prolusione del PROF. L. BIANCHI. G. Civelli, Napoli, 1906. pp. 27.

Anemia in Porto Rico. Preliminary Report. San Juan, 1906. pp. 66.

REPORT OF THE RECENT MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

The Fifteenth Annual Meeting of the American Psychological Association, in affiliation with the American Association for the Advancement of Science, the American Society of Naturalists and the American Philosophical Association was held at Columbia University, New York City, Dec. 27, 28, and 29, 1906. After listening to an address of welcome by Pres. Butler, the first regular session was held at 10.30 A. M., in the psychological lecture-room in Schermerhorn Hall. This session was devoted to a discussion of the subject: Organized Co-operation in Standardizing Psychological Tests. Opinion seemed to be almost unanimous as to the desirability of such standardizing, but there was some difference of opinion as to how best to accomplish it. Professor Angell thought it best to begin with the simpler tests, while Professor Judd would not limit the list to these. Professor Pillsburg suggested that first of all this standardization meant work, hard and long, for all our laboratories before any committee could undertake to prescribe what should be done. The question was left in the hands of a committee of seven to report on the advisability at a later session. Their report favored placing this work in the hands of a committee of five and it was so adopted.

On Thursday afternoon the papers were as follows: *Professor Simon Newcomb: The Estimation of Visual Space Magnitudes.* The speaker brought a wealth of fresh examples to illustrate the accuracy of estimation of length, how we use the angle subtended by the object and yet are not accurate in our estimation of angles alone, and of the fact that we depend largely on internal images, by which he would mean what psychologists express by the subjective element in visual space-perception. Prof. Baldwin pointed out that instinct could furnish animals, such as the chick, with this subjective basis for accurate space perception. *F. Lyman Wells: A Scientific Criterion of Literary Merit.* Ten literary men of America were rated by the members of a graduate club of Columbia University. Some of the points graded were imagination, clearness, finish, euphony, and wholesomeness. Such a study tells us less about the men rated than of the men doing the rating. It also shows that this group of literary men is more homogeneous than the scientists studied in a like manner a few years ago. It tells us little of those falling in the middle group but more of those in the lower and higher. *George M. Stratton: The Photography of Ocular Movements.* The speaker uses the reflection of a point of light on the eye in recording ocular movements. Prof. Judd's excellent work, he feared, would lead to the inference that his method, the placing of a particle of white on the eye and photographing the movement of that, was the only method possible. Actual negatives were used as slides to show the irregular movement of the eye in tracing a square, a circle, an imaginary circle, etc. In discussion Prof. Judd maintained that his method allowed of better interpretation of results. *Raymond Dodge: Minimum Exposure in Experimental Studies of Reading.* It is a mistake to reduce exposure times in reading tests. Eye movement is relatively slow. Reduced time does not necessarily limit physiological excitation and the mental processes are not thereby made simple and parallel. Illumination of the pre- and post-exposure fields may modify results. *J. Carleton Bell: Studies in Binocular Depth Perception.* The tests were made in the Harvard laboratory by means of two dots of light which by means of screws seemed to approach the subject as they were brought together or recede as they were separated, and could be combined stereoscopically. The author concludes that binocular depth perception is a function of accommodation and convergence. In many cases, however, there is a greater or less dissociation of the two when accommodation plays the chief rôle. The subject deserves more attention than it has hitherto received.

Following a brief recess came the address of the President of the Psychological Association, *James R. Angell*, subject: *The Province of Functional Psychology.* At present one can do little more than state a programme, and delimit the scope of this kind of psychology. It is chiefly an attitude of mind. Really it is as old as Aristotle, Darwin and Spencer. Its chief contrast with Structural Psychology is in the fact that the latter deals with states of consciousness which for its analysis are more or less artificial. Structure can never be twice the same while function may be. The *why* and *how* are always implied in the *what*, so that structural may include only a part of the problem of psychology while functional more easily includes all three questions. Biology has given a more and more prominent place to mind. Pedagogy finds much more that is useful in looking at mind from the functional point of view. So also does mental Hygiene and Psychiatry. The recent and large amount of work done in Animal Psychology on the subjects of Instinct, the senses of animals and modifiability, form a significant chapter in functional psychology. Human Genetic Psychology adds another chapter in its attempts at a longitudinal section of consciousness and a correlation of this with growth. The leaning of the Functionalists toward Biology is well calculated, of course, to arouse the ire of the pure psychologists and yet Functionalism deals with that which is vital to the organism. The fundamental conscious utilities are instinct, the short circuit algedonic, and the long circuit algedonic, reactions. The author omitted most of his detailed classifications which illustrate his notion of the usefulness here of the functional point of view. The problem of mind and body he would regard as largely methodological. Behavior and control is the chief point of interest in the living organism. The functional point of view is calculated to unify and make more nearly one many of our subjects of study which now seem so far apart. *Charles H. Johnston: Feeling Analysis and Experimentation.* The meagre result obtained in our attempts at analysis of feeling has been due to the over-emphasis of the cognitive side as well as to the fact that we have viewed the feelings from the structural point of view and have depended too much on the analytic method. We do not know whether feeling (affective state) is an independent element, a complex process, or an accompaniment of something else. Yet we all mean somehow the same thing. The impression method seems most favored and yet our introspective data is not reliable. The pleasant and unpleasant category seems certain, from recent experiments which have largely been made to test Wundt's tridimensional theory. *John F. Shepard: Some Results of Experiments on Cerebral Circulation in Sleep.* Very sensitive brain plethysmographs were used. Marked changes in the curves were shown to be produced by awakening the subject or by simply disturbing him. There is some head movement during sleep and this modifies the curve. This was obviated by placing the head in a swing. The Traube-Hering breathing curve is very marked when the subject is asleep. The results were striking because they show a marked increase in brain volume with depth of sleep though there may be at first a slight decrease. *Stuart H. Rowe: The Difference Between a Habit and an Idea.* For practical application to teaching these should be sharply distinguished. The paper dealt with several contrasts which obtain between them. *George H. Mead: The Relation of Imitation to the Theory of Animal Perception.* Hobhouse's theory of Imitation was criticised. Perception in Animals seemed to involve considerable experience of kinæsthetic value. In making experiments to test for Imitation this should be taken into account. *John B. Watson* (reported by Mr. Carr): *Kinæsthetic Sensations: Their Rôle in the Reactions of White Rats to the Hampton Court Maze.* Rats were first taught to go through the maze and then their eyes removed. On being returned to the maze they showed no disturbance of any sort. The ears, vibrissæ, face, olfactory organs, soles of the feet—in fact, all the organs of the animals except those of kinæsthetic value—were removed or made useless. The animals showed little if any loss in ability to learn or relearn the maze. The maze was swept with air currents in different directions and cold and warm linings were placed at the places of turning but without effect. Quite a disturbance was caused however, by turning the maze through 90°, 180°, and 270°. This last is entirely in agreement with the present writer's work on English Sparrows and other birds. *H. S. Jennings: Habit Formation in the Starfish.* In order to get proof of habit formation one must, so to speak, corner the animal. This is very difficult with the starfish for the reason that it is so versatile, *i. e.*, it can accomplish the same thing in so many different ways. By not allowing it to right itself by turning under those rays which it was seen to use most habitually it was found possible to make it use others. This habit had only partially disintegrated after an interval of three or four days. *Robert M. Yerkes: Modifiability of Behavior in the Dancing Mouse.* By giving these animals an electric shock when they enter the wrong opening they may be taught to distinguish between openings surrounded by black or white cardboard. The males learned more rapidly. Red has not the same value for them as for us. Nine-tenths of all color tests on animals do not require color discrimination by the animals. The reviewer cannot at all agree with the statement that an electric shock is a better *motif* to use with animals than a natural degree of hunger. He is in hearty agreement with the standardizing of mazes to be used with animals. *James P. Porter: Further Study of Variability in Spiders.* This paper gave further material gathered since the report of a year ago and was illustrated with lantern slides.

At 3 P. M., on Friday, President Butler gave his address of welcome to the Philosophical and Psychological Associations in joint session. He emphasized the great need for careful practical work in these fields in view of the great waves of feeling which sweep at times over our democracy, and also that few know what real thinking is. Then followed the address of *Prof. James on "Men and Their Energies."* He referred to Prof. Sanford's insistence on the physician's attitude of mind and to the more adequate treatment which Prof. Janet has given to abnormal conditions because, for one reason, he has not expressed or thought them in the ready-made psychological terms. Few of us put forth all the effort of which we are capable. There are different levels, and habit fixes the amount of energy we expend. We can in physical effort get our second wind and a third and fourth. This is true of the mental life also. The stimuli of war, and of love, serve to move us to greater and greater effort. The letters of the Englishman in charge of the city of Delhi at the time of a siege illustrate in a striking manner the rising to higher and higher levels. Janet's psychoasthenics are examples from the field of pathology. The decisions, moral and otherwise, required of a city dweller demand an output of energy which is not at all called for from the countryman. Prof. James's European friend, who for many years had suffered from a collapse every fourth week finally set for himself the task of the Yoga ordeal. The speaker quoted at length from letters to describe the fasting, prayer, postures and reduction of breathing, etc. After one severe collapse there seems to have been a complete cure. It may be all self-suggestion, but he can now undergo the most trying hardships. In all the above considerations and many more the problems which face us are: (1) What are the problems of mental work? (2) What really are our powers and capabilities? and (3) What are the best means of unlocking these?

I. Madison Bentley: The Effect of Distraction Upon the Intensity of Sensation. So far as the experiment has been carried the effect has been an underestimation of both the loud and weak sound stimuli. *C. E. Seashore: Some Contributions to Tone-Psychology.* There is a surprisingly wide variation in Pitch discrimination. Subjects from college and high school are no better than grammar grade pupils. The determining factor is probably physiological. Musical training modifies it but little. The speaker made certain classifications several years ago; to representatives of some of these it is inadvisable to attempt to teach music at all. *E. H. Cameron: Tonal Reaction.* The apparatus consists of a diaphragm attached to a pen the vibrations of which are written on a moving paper strip. By means of a time line these vibrations may be measured. Subjects with and without musical training start low and waver about a tone which they are asked to sing. In imitating an organ-pipe tone they do the same. The effect of another tone sounded as they attempt to imitate is to render them more inaccurate.

At the dinner of the American Society of Naturalists and Affiliated Societies an excellent address was delivered by Dr. Davenport. After reviewing the co-operative work by the scientific societies of the past the speaker made a plea for such at present in the wide field of Biology. First and incidentally, for the fixing of nomenclature; secondly, and of much more importance, for co-operation in the prosecution of research.

At another session of the Psychological Association the following papers were presented: *Eleanor Harris Rowland: A Proposed Method for Teaching Æsthetics.* The teaching of this subject in our colleges is in an unsatisfactory condition. If the subject is given some attention in the grades and high school, it receives so little in our colleges that the best among the students do not follow it further. In Boston there is a plan to be put into operation which will involve visits to the museum, the lending to colleges of works of art, and lectures to the students by museum officials and artists. The students are to see works of art and then discuss principles. *W. B. Pillsburg: An Attempt to Harmonize the Current Psychological Theories of Judgment.* There are four current theories. That of Brentano in which belief is the central factor. For Marvin it is equivalent to comparison. Again Judgment may be said to be a sort of evaluation; while the last tendency makes it identical with the ascription of meaning. The last three were shown to be much the same while belief is concomitant with judgment. Prof. Dewey's element of doubt was not considered by the speaker to be an important consideration. *Kate Gordon: A Classification of Perceptial Processes.* We are to get our basis for a new classification in the motor response. Intensity, extensity and duration of sensation are to be related to the same. Quality gets its character here also. Schneider's expansion and contraction theory was invoked as explaining much, but discussion did not seem to favor it. *A. H. Pierce: Imagery Illusions. The Non-Visual Character of the "Proof reader's Illusion."* The author criticised the explanation of the "Proof reader's Illusion" found in the usual psychological texts. For him such is not visual at all, but auditory or articulatory. The phrase Imagery Illusions was proposed. *R. S. Woodworth: Non-Sensory Components in Sense Perception.* The way a subject sees the stair-case figure is not due to eye-movement, to the part fixated or to the eye-balls. This is true of other equivocal figures. Subjects agree in saying that preceding images do have a determining influence. Hence what is seen is non-sensory, it is a percept quality and is further illustrated by the way we perceive size, distance, and various rhythms. A "Mental Reaction Theory" was proposed to explain how we get this peculiar quality. It is not the result of motor response or a synthesis of sensations. This theory is probable from a consideration of brain structure. *E. L. Thorndike: The Mental Antecedents of Voluntary Movements.* Any mental state may be the antecedent. The opponents to this view were asked to consider the following: What sort of images do you have when you will not to perform an act? Suppose you will to move your eyes in a straight line across the page. You never have done so and never can. Will to copy several pages of a book or article you have written. What is antecedent to involuntary movements? The view upheld by the other side makes voluntary and involuntary movements radically different. Considerable and interesting discussion was called forth by the last four papers read.

Receptions were tendered the visiting scientists by the President and Trustees of Columbia University, and by the College of the City of New York, in the splendid new buildings of the latter. On Saturday afternoon, the unveiling of ten marble busts of pioneers in American Science at the American Museum of Natural History and a reception in the evening formed a fitting close to a most profitable and enjoyable series of meetings. JAMES P. PORTER.

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DEMONSTRATION OF FORMULÆ FOR TRUE MEASUREMENT OF CORRELATION.

By C. SPEARMAN.

It seems daily more evident, that one of the most important tasks awaiting psychologists is the accurate measurement of the 'correlations' (*i. e.*, the tendencies to concurrent variation) between psychical events, qualities, faculties or other characteristics. For this purpose a now well-known method of calculation has been evolved by Bravais, Galton and Pearson, which furnishes a numerical 'coefficient' measuring precisely the degree of proportionality between any two series of values; this coefficient is usually denoted by the symbol r.[1] Should on any occasion the correlation between the two series take some more complicated form than that of simple proportion, then r has to be supplemented by further terms to express such correlation completely; r remains, however, the principal term (with or without some unessential modification of outward shape).

Unfortunately, there is a considerable step between arriving at a coefficient of correlation and discovering the *true* coefficient. In the first place, the values immediately attainable by investigation are not those of the characteristics really investigated, but only those of measurements, and—in the case of psychology—for the most part very fallible measurements. Secondly, it is usually quite impossible to keep the investigation clear of many factors that do not properly belong to it. The actual effect of these two disturbances, the observational errors and the irrelevant factors, is not merely to diminish somewhat the accuracy of the calculation, but to render the apparent correlation (whether calculated or merely casually inspected) wholly untrustworthy. A large correlation may be obliterated; an illusive one may be conjured up where none exists really; it may even happen that a positive correlation is turned into an apparently negative one, or *vice versa*.

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[1] For method of calculation see this *Journal*, 1904, XV, pp. 77-8 (but for "median" substitute "average").

Now, two formulæ were given by me in this *Journal* some time ago,[1] whereby the effect of both these disturbances can, as I believe, be eliminated. The formulæ were, however, not accompanied by any *proofs*. So many other mathematical formulæ were given at the same time, that the formal demonstrations of them all would have made the article exceedingly cumbersome.[2] But since then I have repeatedly been asked for these proofs; some mathematicians have gone so far, as to doubt whether such proofs could possibly be valid. It therefore seems advisable to publish them.[3]

For convenience of demonstration, I will commence with the formula for eliminating irrelevant factors, although in application the other formula must be used first (to *all* the coefficients entering into the former formula).

1. *Proof of the formula for eliminating the effect of irrelevant factors.*[4]

Let X, Y, and Z denote the values of any three variable and correlated characteristics of objects of any particular class (for instance, their height, length and breadth). Let their average values be denoted by a_x, a_y and a_z respectively; let $a_x - X = x$, $a_y - Y = y$, and $a_z - Z = z$. Further, let b_{xy} and b_{xz} be such values, that $\Sigma\,[\,x - (b_{xy}\,y + b_{xz}\,z)\,]^2$ is a minimum. Equating to 0 the differentials of this sum with respect to both b_{xy} and b_{xz}, and solving these two equations for b_{xy}, we find

$$b_{xy} = \frac{\Sigma\,xy\,.\,\Sigma\,z^2 - \Sigma\,xz\,.\,\Sigma\,yz}{\Sigma\,y^2\,.\,\Sigma\,z^2 - (\Sigma\,yz)^2} = \frac{r_{xy} - r_{xz}\,.\,r_{yz}}{1 - r_{yz}^2}\,.\,\frac{\sqrt{\Sigma\,x^2}}{\sqrt{\Sigma\,y^2}}$$

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[1] Vol. XV, 1904, pp. 88-96.

[2] As an example, it may be mentioned that another formula given in the same article has just had to be demonstrated also. The simple statement of values, as originally given in this *Journal*, took up only a couple of lines; whereas the mathematical demonstration (in the *British J. of Psych.*) fully occupies three pages.

[3] These formulæ are again much utilized in a paper of Professor Krueger and myself in the *Zeitschrift für Psychologie* (44, p. 48).

[4] Parts of the printer's proofs have very kindly been looked over by Drs. Herglotz and Carathéodory (lecturers on mathematics, University of Göttingen) and Dr. Ehrenfest, who have furnished valuable criticisms.

where r_{xy} denotes Pearson's correlational coefficient

$$\frac{\Sigma\, xy}{\sqrt{\Sigma\, x^2 .\ \Sigma\, y^2}},$$

and r_{xz} and r_{yz} have similar meanings.

Analogously $\quad b_{yx} = \frac{r_{xy} - r_{xz} \,.\, r_{yz}}{1 - r_{xz}^2} \cdot \frac{\sqrt{\Sigma\, y^2}}{\sqrt{\Sigma\, x^2}}$, so that

$$\sqrt{b_{xy} \,.\, b_{yx}} = \frac{r_{xy} - r_{xz} \,.\, r_{yz}}{\sqrt{(1 - r_{xz}^2)\,(1 - r_{yz}^2)}} \qquad (a)^{1}$$

Let us next suppose our whole class of objects to be split up into groups, a group embracing all those objects for which Z has any constant value. Let us apply the considerations and notation of the preceding paragraph to the kth such group apart. The term b_{xz_k} vanishes, since z_k clearly $= 0$.

We get, then, $\Sigma(x - b_{xy_k}\, y)^2 =$ a minimum, from which $\frac{d}{d\, b_{xy_k}} \Sigma\, (x - b_{xy_k}\, y)^2 = 0$, so that $b_{xy_k} = \frac{\Sigma\, xy_k}{\Sigma\, y_k^2}$

As b_{yx_k} has an analogous value,

$$\sqrt{b_{xy_k} \,.\, b_{yx_k}} = \frac{\Sigma\, xy_k}{\sqrt{\Sigma\, x_k^2 \,.\, \Sigma\, y_k^2}} = r_{xy_k} \,. \qquad (b)$$

Now, in general the value of b_{xy_k} differs from that of b_{xy}; but in three special cases here interesting us it can be shown to coincide.

The first case occurs when the following assumptions are permissible:

$$b_{xy_0} = b_{xy_1} = b_{xy_2} = \ldots = b_{xy_k} = \ldots\ , \qquad (c)$$

$$b_{yx_0} = b_{yx_1} = b_{yx_2} = \ldots = b_{yx_k} = \ldots\ , \qquad (d)$$

$$a_{x_k} - a_{x_0} = e \cdot Z_k \text{ and} \qquad (e)$$

$$a_{y_k} - a_{y_0} = f \cdot Z_k\ , \qquad (f)$$

e and f being constants. It will be convenient to conceive all the objects as represented geometrically by positions having as ordinates X, Y and Z. The equations $x - b_{xy_k} y = 0$ evidently denote what may be called 'minimal lines,' b_{xy_k} being determined for each value of k by the relation $\Sigma\, (x - b_{xy_k} y)^2 =$ a minimum; and all such minima may be regarded as part

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[1] This result was reached, in a somewhat different manner, by Yule (Proc. R. Soc., Vol. LX).

sums of a total minimum which, transforming from x and y to X and Y, may be written as

$$\sum_k \Sigma \left[X - b_{xy_k}Y - (a_{x_k} - b_{xy_k}a_{y_k})\right]^2,$$

or, shorter, as $\sum_k \Sigma (X - b_{xy_k}Y - E_k)^2$, or simply as Σ m.

On the other hand, $x - b_{xy}y - b_{xz}z = 0$ is a 'total minimal plane,' b_{xy} and b_{xz} being determined by the relation $\Sigma (x - b_{xy}y - b_{xz}x)^2 =$ a minimum; and this minimum can be regarded as made up of part sums (not necessarily minima individually), one for each different value of Z, and therefore may be written as

$$\sum_k \Sigma \left[X - b_{xy}Y - (b_{xz}Z^k + a_x - b_{xy}a_y - b_{xz}a_z)\right]^2,$$

or, shorter, as $\Sigma_k \Sigma (X - b_{xy}Y - E^k)^2$,

or simply as M. Evidently, the difference between Σ m and M depends on that between E_k and E^k. But the former is the value that makes every part sum of the form in question a minimum, as may readily be found by determining E from the equation $\frac{\partial}{\partial E}(X - bY - E)^2 = 0$. Hence, if E^k differs from E_k for any single value of k, the corresponding part sum of M becomes greater than that of Σm. *A fortiori*, the total effect of all differences between E^k and E_k for all values of k must be to make M greater than Σm. But in the present case it happens to be possible for all corresponding parts of Σm and M respectively to be identical; for the 'minimal lines,' from which Σm derives, must—owing to the conditions (c), (e) and (f)—lie in one plane; and there is no condition preventing this plane from coinciding with the 'total minimal plane,' from which M derives. Since M *can* wholly coincide with Σm, it *must* do so, for otherwise it would not be a minimum, as required by hypothesis. Hence finally, $b_{xy} = b_{xy_k}$ and, taking into consideration the equations (a) and (b), we arrive at the desired result,

$$r_{xy_k} = \frac{r_{xy} - r_{xz} \cdot r_{yz}}{\sqrt{(1 - r^2_{xz})(1 - r^2_{yz})}} \qquad (g)$$

The second case is where equation (f) is assumed once more and also $b_{xy_0} = b_{xy_1} = b_{xy_2} = \ldots = b_{xyk} = \ldots = 0$. (h) It is clear that all the minimal lines must again lie in one plane, and this can again be shown, by reasoning as above, to coincide necessarily with the 'total minimal plane.' Consequently $b_{xy} = b_{xy_k} = 0$. Hence equation (g) again holds good, either side of it now being equal to 0.

The third case is similar to the first, with the exception, that b_{xy_k} is permitted to vary with k. Under these conditions the equation (g) will be found valid to the extent of giving the most probable mean value of r_{xy_k}.

In equation (g) we have reached at any rate the form of the formula for eliminating irrelevant factors. But now we have to see whether, also, the constituent terms of this equation correspond to the respective values concerned in the actual investigation of correlation. Let us therefore turn to actuality, say, to the correlation found in my previous paper between children's power of discriminating pitch and that of discriminating weight. As irrelevant factor let us take the children's varying age. Clearly, the actually observed correlational coefficients are derived from three variables, discrimination of pitch, discrimination of weight, and age; they correspond perfectly to r_{xy}, r_{xz} and r_{yz} on the right of equation (g). But the factor of age is obviously irrelevant and disturbing; suppose, for instance, that both discrimination of pitch and that of weight improved with age; then of course we should find a correlation between the two sorts of discrimination, for both would be best in the same children, namely, the oldest; such correlation is evidently beside the question. Our true course of investigation would have been to select for experiment children of exactly the same age; but this is precisely the way we arrived at r_{xy_k}: thus r_{xy_k} is the desired *true* correlation between x and y.

It remains to be considered how far the actual observational material fulfils the special conditions under which alone the formula (g) has been shown to be exact. The present topic, that is, the elimination of irrelevant factors, comes under our above 'first case;' we need, therefore, the relations, (c), (d), (e) and (f). Of these the two latter are equivalent to requiring the correlations of the irrelevant term (here, age) with the two main terms (here, discrimination of pitch and of weight) to be linear. This limitation is less serious than might be supposed; for the inexactness of the corrective formula only becomes appreciable when the irrelevant correlations depart from the linear form very largely; and experience has shown such large deviations to be extremely rare. Anyhow, it can scarcely be a matter of surprise that irrelevant correlations become difficult to treat when non-linear, seeing that no quite satisfactory formulæ have yet been discovered even for the bare measurement (*i. e.* antecedently to all corrections) of the main correlation when non-linear.

Finally, the two other conditions (c) and (d), mean that the true correlation must not change appreciably for the different values of the irrelevant term. Now, such changes may be taken as, in general, of a smaller order of magnitude than the alterations to be eliminated by the corrective formula, those produced by *mixing* different values of the irrelevant term. To return to our instance, there is good reason to believe that most correlations are very similar for children of 9 years as for those of 12, although the gravest disturbance will often occur if 9 and 12 years be thrown together into one and the same correlation. Conditions (c) and (d) may therefore be considered sufficiently satisfied whenever the irrelevant influence to be eliminated is of moderate amount. But if, instead of confining our experiments to the ages of 9–12, we had included those down to, say, 5, then the true correlation for 5 years would probably have had quite a large discrepancy from that for 12. In such case one could at most expect any general 'true' correlation to signify the true *mean* correlation; and this, as we have seen, is the value actually given by our formula.

2. *Proof of the formula for eliminating the effect of inaccurate observation.*

We will assume any two correlated series of values, X and Y to have each been measured twice independently, and to have yielded the series of measurements x_1, x_2, y_1 and y_2. The coefficients $r_{x_1y_1}$, $r_{x_1y_2}$, $r_{x_2y_1}$, $r_{x_2y_2}$, $r_{x_1x_2}$ and $r_{y_1y_2}$ can, of course, be reckoned directly. We require a formula to reckon r_{XY}.

Let us first consider the correlations between x_1, x_2 and X, and see how the coefficient between x_1 and x_2 becomes modified when a separate calculation is made for each group of objects for which X is constant, say, $= X_k$. We may fairly assume the average of all the measurements x_{2_k} (or x_{1_k}) to coincide with X_k, or at any rate to vary proportionally thereto for the different values of k; hereby the condition (f) is satisfied. Further, when we consider any k^th group quite apart, since the fluctuations in the two series of measurements x_{1_k} and x_{2_k} of the same value X_k are by hypothesis independent of one another, $b_{x_1x_{2k}}$ (or $b_{x_2x_{1k}}$) always $= 0$, thus satisfying the condition (h). Consequently, we have here our 'second case' and

$$r_{x_1x_{2k}} = \frac{r_{x_1x_2} - r_{x_1X} \cdot r_{x_2X}}{\sqrt{(1 - r^2_{x_1X})\,(1 - r^2_{x_2X})}} = 0, \qquad (i)$$

where X is fixed at X_k on the left side of the equation, but remains variable on the right. Therefore, since r cannot be infinitely great,

$$r_{x_1x_2} = r_{x_1X} \cdot r_{x_2X}, \text{ and analogously} \qquad (j)$$

$$r_{y_1y_2} = r_{y_1Y} \cdot r_{y_2Y} \qquad (k)$$

Next, let us consider the correlations between x_1, X and y_1. If we again make a separate calculation for each group of values for which X is constant, the condition (f) is satisfied just as before. Further, in the calculation for any k^{th} group considered quite apart, X_k, being a constant, is independent of the fluctuations in the measurements y_{1_k}, so that $b_{y_1 x_k} = 0$ and condition (h) is satisfied again. Hence our 'second case' occurs once more and

$$r_{x_1 y_1 k} = \frac{r_{x_1 y_1} - r_{x_1 X} \cdot r_{y_1 X}}{\sqrt{(1 - r^2_{x_1 X})(1 - r^2_{y_1 X})}} = 0. \qquad (l)$$

From this evidently

$$r_{y_1 X} = \frac{r_{x_1 y_1}}{r_{x_1 X}} \text{ and, analogously,} = \frac{r_{x_2 y_1}}{r_{x_2 X}}. \qquad (m)$$

$$\text{Likewise} \quad r_{y_2 X} = \frac{r_{x_1 y_2}}{r_{x_1 X}} = \frac{r_{x_2 y_2}}{r_{x_2 X}}, \qquad (n)$$

$$r_{x_1 Y} = \frac{r_{x_1 y_1}}{r_{y_1 Y}} = \frac{r_{x_1 y_2}}{r_{y_2 Y}}, \qquad (o)$$

$$\text{and} \quad r_{x_2 Y} = \frac{r_{x_2 y_1}}{r_{y_1 Y}} = \frac{r_{x_2 y_2}}{r_{y_2 Y}}. \qquad (p)$$

Finally, let us take the correlations between x_1, X and Y. By reasoning as before, we get

$$r_{x_1 Y_k} = \frac{r_{x_1 Y} - r_{x_1 X} \cdot r_{XY}}{\sqrt{(1 - r^2_{x_1 X})(1 - r^2_{XY})}} = 0,$$

from which it is evident that

$$r_{XY} = \frac{r_{x_1 Y}}{r_{x_1 X}} \text{ and analogously,}$$

$$= \frac{r_{x_2 Y}}{r_{x_2 X}} = \frac{r_{y_1 X}}{r_{y_1 Y}} = \frac{r_{y_2 X}}{r_{y_2 Y}}.$$

By multiplying together the four preceding equations to r_{XY}, we have

$$r_{XY}^4 = \frac{r_{x_1 Y} \cdot r_{x_2 Y} \cdot r_{y_1 X} \cdot r_{y_2 X}.}{r_{x_1 X} \cdot r_{x_2 X} \cdot r_{y_1 Y} \cdot r_{y_2 Y}}$$

Substituting on the right of the above equation from (j), (k), (m), (n), (o), and (p) and taking the real positive root, we find at last

$$r_{XY} = \frac{G(r_{x_1 y_1}, r_{x_1 y_2}, r_{x_2 y_1}, r_{x_2 y_2})}{G(r_{x_1 x_2}, r_{y_1 y_2})}, \qquad (q)$$

where G denotes the geometrical mean.

In practice it will usually be allowable to assume that the two series of measurements of the same series of things have been conducted with equal accuracy. Then $r_{x_1 X} = r_{x_2 X}$ and $r_{y_1 Y} = r_{y_2 Y}$, so that equation (q) becomes

$$r_{X\,Y} = \frac{r_{x\,y}\,(= r_{x_1 y_1} = r_{x_1 y_2} = r_{x_2 y_1} = r_{x_2 y_2})}{G\,(r_{x_1 x_2}\,,\, r_{y_1 y_2})}.$$

The discrepancies that will occur between the four *actual* values of $r_{x\,y}$ must then be attributed to mere chance, and must be met, as usual, by taking an average. Thus we get, on the assumption of the equally accurate series of measurements,

$$r_{X\,Y} = \frac{\dfrac{r_{x_1 y_1} + r_{x_1 y_2} + r_{x_2 y_1} + r_{x_2 y_2}}{4}}{\sqrt{r_{x_1 x_2} \,.\, r_{y_1 y_2}}}.$$

The above proof of the formula for eliminating the effect of observational errors is, as we have seen, exact and perfectly general. It holds good whatever may be the distribution of values, or the size or distribution of the observational errors, in the series concerned and whatever may be the correlation's form. Any discrepancies arise solely from the practical necessity of applying the formula, not to the whole series of values considered, but only to 'random samples' of such series. By sufficiently extending the experiments, the chance of discrepancy may be reduced as much as desired. The formula for irrelevant factors is equally general, except for the two limitations explained above.

Both formulæ concern themselves, however, solely with r, that is, with $\frac{\Sigma xy}{\sqrt{\Sigma x^2 \,.\, \Sigma y^2}}$. But, as mentioned before, when the relation between the two characteristics investigated assumes some special form, instead of the normal 'linear' form of simple proportion, then this special form finds no expression whatever in r taken alone. To express this form analytically, other additional terms are required. The exact nature of these additional terms (as well as the outward shape of r itself) varies somewhat according to the method of calculation adopted. But there is, probably, no mathematical difficulty in devising modifications of our corrective formulæ to suit any such terms.

It should be observed, that in many cases the non-linear form is more apparent than real. Generally speaking, a mere tendency of two characteristics to vary concurrently must be taken, it seems to me, as the effect of some particular underlying strict law (or laws) partly neutralized by a multitude of 'casual' disturbing influences. The quantity of a correlation is neither more nor less than the relative influence of the underlying law in question as compared with the total of all the influences in play. Now, it may easily happen, that the underlying law is one of simple proportionality but the disturbing influences become greater when the correlated characteristics are larger (or smaller, as the case may be). Then the underlying simple proportionality will not appear on the surface; the correlation will seem non-linear. Under such circumstances, r cannot, it is true, express these variations in the quantity of correlation; it continues, however, to express completely the *mean* quantity of correlation.

In the majority of the remaining cases of non-linearity, the latter is merely due to a wrong choice of the correlated terms. For instance, the correlation between the length of the skull and the weight of the brain must, obviously, be very far from linear. But linearity is at once restored (supposing all the skulls to belong to one type) if we change the second term from the brain's weight to the cube root of the weight.

To conclude, even when the underlying law itself really has a special non-linear form, although r by itself reveals nothing of this form, it nevertheless still gives (except in a few extreme and readily noticeable cases) a fairly approximate measure of the correlation's quantity.[1]

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[1] Several writers, who have made otherwise valuable contributions to the subject of correlation, but have been too exclusively guided by the purely mathematical point of view, appear to have wholly overlooked this fundamental distinction between the form and the quantity of a correlation.

THE SIGNIFICANCE OF WAVE-FORM FOR OUR COMPREHENSION OF AUDITION.

By MAX MEYER.

A recent number of the *Zeitschrift für Psychologie*[1] contains a set of curves representing the resultants of two sinusoids each for all the ratios made up of the numbers from 1 to 12. It contains, further, the resultant curves of the ratio 2:3 for various phase-differences, and the resultant curves for the ratios 4:5:6, 4:5:9, and 4:7:9. In the accompanying article, Professor C. Stumpf discusses these curves with an aim indicated by the following remarks. There is no doubt that the Helmholtzian theory of audition in its traditional form is imperfect. In order to modify it, we ought to study the characteristic properties of the various forms of compound curves. Then only can modifications of the theory be worked out. Even those who believe in the theory of resonance quite literally, will be able to obtain from a study of such curves material for the criticism of theories which are not based on the assumption of resonators in the ear.

The present writer took a special interest in this paper, because many years ago he studied the same curves with the same aim. He was soon led, however, to a point of view different from that of Professor Stumpf.

In the first place, there arises the general question: What is the use of *mathematical* speculations about wave forms? They will never be of any use whatsoever, unless they can lead to some idea about the *mechanical* processes taking place in the organ of hearing, in the cochlea. Now, to obtain such an idea we have to search in a region where there are no previously established paths. The only aids for finding our way in this darkness are the following observations:

1. Auditory stimuli, according to a general agreement, consist of *oscillations*. But nothing compels us to assume *apriori* that oscillations must be of the form of sinusoids in order to stimulate the auditory organ. *They might be of other forms.*

2. When we look at a compound curve, *i. e.*, a curve made up by the geometrical addition of, say, two sinusoids, *it looks* to us

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[1] Zeitschrift f. Psy. u. Physiol. d. Sinn. XXXIX, p. 241-268.

like a sum of several oscillations (of whatever *form* they may be), just as *we hear* a sum of several tones. By comparing various ways of *looking at curves* with the tones we actually *hear*, we might discover that some ways of looking lead to a greater resemblance than others between what we see and what we hear.

3. Having found a way of looking at curves which leads to a considerable resemblance between what we see and what we hear, we should try to find out whether our organ of hearing could *function mechanically* in the manner indicated by our way of looking at the curves. If we discover such a function, then our problem is practically solved.

Upon the general principles just stated there seems to be agreement between Professor Stumpf and the writer. There is no agreement, however, upon the special paths by which to proceed in the application of these principles.

1. *What is an oscillation?*

Professor Stumpf and the writer agree that a definition of "an oscillation" must be altogether a matter of utility. But the writer cannot accept the use of the "middle line" for a definition. On the contrary, he has long believed that this use narrows down the possibilities of looking at curves to such an extent that we can never hope, by its means, to reach a satisfactory agreement between what we see and what we hear.

By "middle line," Professor Stumpf means the horizontal co-ordinate when so placed that all the points of inflection of the original sinusoids come to lie on it. And by "an oscillation" he means (p. 244) a part of the compound curve which starts from a point on the "middle line," returns to the "middle line," passes over to the other side of this line, and returns a second time to a point upon it. The part of the curve from the starting point to the second crossing of the middle line is "one oscillation." He emphasizes the fact that it is our purpose in a study of such curves to *count the oscillations*, thus or so defined. But he does not say how we should define and count oscillations in case the curve moves towards the middle line and then away from it *without touching it*. This course occurs in innumerable instances, when the amplitudes of the original sinusoids are unequal. In such a case, we could not simply omit the up-and-down movement in question, or we should get a collection of tones which in reality no one hears. The writer is, naturally, very far from asserting that a definition of an oscillation is altogether valueless, because it is applicable only to the special case of equal amplitudes; but he believes that, when all the facts are considered, Professor Stumpf's definition by means of the middle line must be pronounced inferior to that which he himself published in the

Zeitschrift, Vol. XI, p. 216-217 (1896). In the writer's definition, Professor Stumpf's "middle line" plays no part. The question, therefore, need not be raised whether a maximum or minimum reaches beyond or touches the "middle line," or fails to do so. What is of primary importance, in the writer's definition, is the *ordinate differences* of the maxima and minima, not the ordinate values themselves. The absolute ordinate value of a single maximum or minimum (referred to in Professor Stumpf's "reaching beyond," "touching," or "leaving untouched" the "middle line") should not enter into a definition, because the location of the horizontal co-ordinate is arbitrary and, therefore, cannot possess any mechanical significance.

This, perhaps, is the chief difference between the writer's attitude toward the present problem and the attitude of Professor Stumpf: that the former insists always upon the possible mechanical significance of the oscillations. In the publication above mentioned, the writer showed (p. 225) the possibility of such a mechanical significance, although he did not then succeed in making this particular mechanical application appear very probable from an anatomical point of view. Professor Stumpf, however, is apparently satisfied with definitions of oscillations as such. He leaves the mechanical side of the problem to the future. He seems to imply (p. 255, line 6, "Endlich") that there is to be found here a collection of *all possible* definitions of "oscillations," so that the future investigator must choose among the five given. But in reality, the number of such definitions, since they are, naturally, not logical deductions from a single principle but arbitrary inventions for purposes of utility, might be largely increased. In particular, the definition of "an oscillation" developed by the present writer is not mentioned. Now, since *all* such definitions of "oscillations" are originally equally arbitrary, it would appear that those and those only should be selected for discussion which evince a certain amount of harmony with the facts of auditory observation and which can be shown to possess a certain mechanical significance. The remarks by Professor Stumpf on pp. 266-268 do not seem to the writer a fulfilment of these two conditions.

The definition of "oscillation" just spoken of is mentioned by Professor Stumpf on p. 254 under (2). He mentions under (1) another definition, which regards the whole period of the sum of the two sinusoids as a single oscillation, but discards it as inadequate. Let us, then, turn to the three remaining definitions. The third, which again makes use of the "middle line," is this: "An oscillation is the double length of the first section of the middle line in case $\frac{H}{L} \leqq 3$, the double length of the sum of the first two sections of the middle line in case $\frac{H}{L} > 3$. The frequency of oscillation is then $\frac{H+L}{2}$."

The fourth definition is this: An oscillation is the period from one relatively highest (*i. e.*, having lower ones at either side) maximum to the next relatively highest maximum of the curve. The frequency of oscillation is then equal to H—L in case $\frac{H}{L} \leqq 2$, equal to L in case $\frac{H}{L} > 2$.

The fifth definition is this: An oscillation is generally identical with the component sinusoid of lowest frequency. The frequency of oscillation is then simply L.

II. *What definition is the most promising?*

It is clear that all definitions of "an oscillation" have for their immediate purpose the finding of an agreement between the result of our "looking" (perhaps it would be better to say "counting oscillations by looking") at curves and our auditory experiences. The agreements found here are the following:

Looking at the "middle line," in accordance with the second definition, may "explain" to us why we can hear the *higher* one of the two original tones. (*H.*)

Looking at the "middle line," in accordance with the third definition, rather contradicts than agrees with observation; for we do not universally hear a *mean tone* $\frac{H+L}{2}$. We hear such a tone only with very special and rather rare ratios (very small intervals). (*M.*)

Looking at a curve in accordance with the fourth definition may "explain" *one difference-tone* in cases where the interval is less than an octave. It does not indicate the fact that we can often, indeed usually, hear more than a single difference-tone. As regards difference-tones of larger intervals, the definition reveals only the negative fact that the tone H—L is there not ordinarily audible, but nothing positive; although in fact certain difference-tones are nearly always clearly audible in such intervals. (*D.*)

Looking at a curve in accordance with the fifth definition may "explain" why we can hear the *lower* one of the two original tones. (*L.*)

It is plain, then, that for the sake of an "explanation" of the most ordinary phenomena of audition (1. hearing *H*, 2. hearing *L*, 3. hearing *one D*), we should have to apply to a given curve at least *three different definitions of "an oscillation" at the same time;* if we wished to include the "mean tone," *even* *four different definitions at the same time!* It is not likely that anything of scientific value will be gained in this manner.

For comparison, the writer's *single* definition of "an oscillation" is translated here from *Zeitschrift* XI, p. 216: "Find the smallest ordinate difference between a maximum and the preceding minimum (or the reverse; the order is irrelevant), and cut off from each ridge and each trough of the wave a piece whose height is equal to one-half of this difference. Each pair of a higher and a directly following lower segment of this kind counts as "one oscillation." The height of the segments is to be regarded as a measure of the intensity of the tone. Apply the same rule to the wave remaining, and so on until the wave is reduced to a straight line."

This *single* definition of an oscillation "explains" more than all the present five together; it explains, *e. g.*, the fact that we can hear several difference-tones at the same time. Of course, one must not expect the definition to explain every detail of audition; that is, in any case, no serious matter. A definition of this kind is not an end in itself, but merely a means to an end; the end being the discovery of the mechanical function of the inner ear. This leads us to the third point of our discussion.

III. *The mechanical significance of the writer's "oscillation."*

How does the inner ear function mechanically? All studies of curves and wave-forms, all definitions of oscillations and their relations to the facts of audition, are mere means, tools, aids in the approaching of this problem. And therefore, any definition of "oscillation," any manner of looking at compound curves, is scientifically valuable only in so far as it suggests a definite mechanical process which must be found possible under the anatomical conditions of the cochlea. Now Professor Stumpf's paper gives no suggestion of a mechanical process corresponding to any one of his definitions of "oscillations." On the other hand, having found the definition above mentioned, the writer attempted to find a mechanical process which was in accord with it. Such a process is described in *Zeitschrift* XI, p. 225, directly following the definition of "oscillation." The mechanical process, however, which he was at that time able to discover, although unobjectionable from a purely physical point of view, offered a considerable anatomical difficulty. It assumed an imperfectly elastic rod, fastened to a support at the one end, the free end moving transversely in the form of the compound curve. But what anatomical structure could be a rod of such properties? This was the unsatisfactory part of the assumption.

However, a few months after this publication, the writer hit upon an idea which led to the unfolding of what he now regards as the correct—though incomplete—theory of the mechanical function of the inner ear. The anatomical difficulty was solved. This solution is published in outline in *Zeitschrift* XVI, p. 22. He found that the following mechanical process would correspond exactly to his definition of "oscillation." Imagine a relatively long and narrow tube, divided lengthwise, by a flexible but perfectly inelastic band or partition, into two tubes of approximately semi-circular cross-section. The partition, being inelastic, could not, when displaced, return to its normal position by its own force, but only under the influence of external forces. Imagine this partition to be restricted in its movement, so that in either direction it can yield to pressure only within narrow limits. Imagine, further, this tube to possess at the one end two windows closed by membranes, one window for each division of the tube. Imagine a piston-like body to be attached to the membrane of one of the windows. Now, when this piston-like body moves back and forth in the form of any given compound curve, we shall observe on the successive sections of the partition "oscillations" which are exactly like those above defined. Details of this view or "theory" may be found in the paper mentioned and in other publications of the writer's. Here a few further explanations may be added. Under the conditions assumed, the length of the section of the partition which—point after point—yields to the movement of the piston must always be proportional to the distance through which the piston moves in one and the same direction. And whenever the piston reverses its movement, *begins* to move in either direction, the point of the partition which first yields must be that nearest the windows; the other points yield later. This last principle may be exemplified by the fact that a gun barrel sometimes explodes although the front end is open; in the same way the initial sections of the partition yield before those more distant.

The anatomical application of this mechanical function is at once clear. The cochlea is a relatively long and narrow tube. It is divided by a flexible partition into two divisions. Each division has a window at the one end of the tube. And one of these windows contains a piston-like body, the stirrup.

But there is one seeming difficulty left. The partition in the cochlea, although undoubtedly flexible, *cannot be regarded as perfectly inelastic.* But this, far from being a serious difficulty, explains what otherwise would be another difficulty. The partition is certainly not perfectly inelastic; and it must be its very elasticity which confines its yielding movements within the limits above spoken of. Not, of course, that by assuming a certain degree of elasticity of the partition we relapse into the resonance theory, the piano-in-the-ear doctrine:

we assume some elasticity, but *no tension*. Indeed, it is not easy to see how tissues which are under *constant* tension during the whole life of the individual could retain their tension, and not simply grow longer, adapt themselves, and thus lose their tension. This is what happens wherever constant tension is observed in growing, living tissues, vegetable or animal.

The task now left is to work out the mechanics of the inner ear in detail, making use of all the anatomical facts at our disposal. The writer has worked out, to some extent, a very few of these details, and has published the results in various papers. However, we still have only the barest outlines of a mechanics of the inner ear.

It has been mentioned that the writer's definition of "an oscillation" does not agree in every detail with the facts of audition. It has now become clear why there could not be a perfect agreement: the mechanical theory which is strictly equivalent to that definition is—from the anatomical point of view—*only a close analogue of the mechanics of the inner ear*, not its true representative.

The progress of the mechanical theory, if such progress should result from an increased interest in this matter among scientists, is sure to have a beneficial influence on our purely psychological knowledge of the facts of audition. Whoever has attempted to familiarize himself, *e. g.*, with the results of such otherwise admirable work as that of Krüger on difference-tones, must have been convinced of the enormous waste of mental energy resulting from the fact that these experiments were scarcely made with definite questions in view. Experiments which are not made on the basis of a definite theory may nevertheless lead to valuable discoveries; but they are more apt to lead to the collection of a mass of material so large that no one can mentally digest it. With the progress of the theory, there will also come a new advance in experimentation.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE.

Communicated by M. F. Washburn.

III. A Study in the Comparison of Time Intervals.

By Edith A. Alvord and Helen E. Searle.

The object of the experiments to be described was to investigate the processes which occur in the mind of a person who is asked to estimate the length of one interval of time in terms of another. No attempt was made to test, except roughly, the accuracy of such estimations, and the conditions were purposely left as nearly like those of our everyday time judgments as possible. The intervals used as standard varied from three to twelve seconds, and were thus much longer than those employed in 'time sense' experiments. The weight of the study rests upon the introspective testimony of the observers as to their methods of judgment.

The observer sat in as quiet a room as could be obtained. The experimenter marked off the intervals by striking a telegraph key at the beginning, at the end of the first or standard interval, and at the end of the second interval. The latter was made from ten to twelve times as long as the first, and followed directly after it, one and the same click of the key marking the end of the first interval and the beginning of the second. The observer was asked to state at the end of the experiment how many times the first interval was contained in the second. The only direction that was given her was that the times were not to be measured by filling them with counting; she was at liberty to use any other method that seemed natural. Furthermore, the length of the standard interval was varied so that a shorter and a longer one were given alternately.

The five observers, all fairly practised in introspection, differed decidedly in the methods they adopted, and may be considered separately.

Observer S., in by far the greater proportion of the experiments upon her, judged the length of the standard interval in terms of muscular strain and relaxation, and that of the comparison interval by noting how many times the strain reached the same degree of intensity as at the end of the standard.

The strain and relaxation were expressed by S. in different ways, sometimes by the effort of climbing imaginary stairs and dropping back at the end of the unit interval; sometimes by thinking of herself as running a race and stopping now and then; sometimes simply in terms of tension in the head. The first forty experiments alternated a standard of five seconds with one of ten seconds; the second forty, a standard of three seconds with one of six seconds. In all the statements of results given below, the amount of the average error is mentioned, together with its sign. Thus when for the five second standard the error is said to be +.5, this means that while the longer of the two intervals was really twelve times the standard, D.'s average estimate of it in twenty experiments was 12.5. Thus the standard interval, as reproduced by D. during the longer interval, was slightly underestimated, since more of the reproduced standards were put into the latter than were really contained in it. In like manner an error with negative sign means an overestimation of the standards.

The average errors of S. were, for a standard of 5 sec., +.5
For a standard of 10 seconds, +2.0
“ “ “ 3 “ —.8
“ “ “ 6 “ +1.2

It will thus be seen that she overestimated the shortest interval, 3 seconds, slightly underestimated an interval of 5 seconds, and underestimated to an increasing degree intervals of 6 and 10 seconds. In the next set of forty experiments, made with standards of four and eight seconds alternately, S. made much less use of tension and relaxation and more use of a kind of auditory rhythm than that 'ran through her head.' Sometimes this rhythm became a tune, but oftener was without marked pitch differences. Her errors were:

For a standard of from 4 seconds, —.1
“ “ “ 8 “ +.8

The great accuracy of S.'s estimation of the four second interval is noteworthy. But more especially to be observed is the fact that the underestimation of the eight second standard is considerably less than that of the six second standard. In other words, while in general one might say, judging from these rough results, that S. had a tendency to overestimate intervals below five seconds and underestimate intervals above that limit, she underestimates less when she uses an auditory rhythm to judge by than when she uses strain and relaxation.

Observer Ald. did all her measuring in motor terms, not involving tension and relaxation, but merely imagined movement, probably a sort of rhythm with decided motor elements. She imagined her eyes moving along a line, her hand writing, her head bobbing up and down, her feet walking. The experiments followed the same plan as with S. The following are the average errors:

Standard:	5 seconds.	Error:	—1.8
"	10	"	"	+1.6
"	3	"	"	—2.8
"	6	"	"	—.5
"	4	"	"	—3.7
"	8	"	"	—1.05

Every standard except that of ten seconds was overestimated.

For observer W., the intervals measured themselves by an imagined auditory rhythm, and within the long interval, at the end of each period of time which was judged equal to this standard, the rhythm series came to an end with an auditory image of the count, 'three,' 'five,' etc. The standard itself was not filled with any rhythm; the latter set in usually shortly after the beginning of the large interval. The rhythm was during the first part of the long interval entirely independent of breathing, which was often quite irregular, but towards the end of the time the breathing tended to fit itself into the rhythm already established. A number of times it appeared to W. that the beats of the rhythm were synchronous with heart beats, and this was approximately verified by feeling the pulse at the close of the experiment; in other cases the rhythm seemed to be faster or slower in a way which the heart beat could scarcely account for. The following were the results for W:

Standard:	4 seconds.	Average	error,	—3.7
"	8 "	"	"	—1.05
"	6 "	"	"	—.1
"	10 "	"	"	—.4
"	5 "	"	"	—.1
"	13 "	"	"	+2.8

It will be seen that W. resembles A. in overestimating every interval except the very longest one; and that even the ten second interval, which A. did shorten in reproduction, was lengthened by W. These results surprised W. herself, for she felt always in the case of the longer standards that her reproductions of them were too short. W. made almost no use of strain and relaxation as a method of measurement.

In observer F. still another method of reproduction showed itself. At the end of each of the first three to seven periods, within the longer interval, which was judged equal to the standard, a more or less distinct auditory image of the click ot the key presented itself, as it were automatically, and the observer's attitude was simply that of waiting for this imaginary sound. The image accurately reproduced any slight peculiarity in the click such as was occasionally produced by the experimenter's not striking the key quite squarely. Sometimes as many as seven imaginary clicks were heard during the long intervals, but they tended to become less distinct as they were further removed from the original sound. When they did not occur, strain and relaxation sensations seemed to be used for measurement, and these were also experienced with the sound image. Once F. said there seemed to be "a contraction until the sound image came and then an explosion," and another time she said that "everything pulled towards the sound." Her results were as follows:

Standard:	5	seconds.	Average	error,	—.6
"	10	"	"	"	+2.4
"	6	"	"	"	—.9
"	8	"	"	"	+.3

We see again here the tendency to overestimate the shorter intervals and underestimate the longer ones, which was displayed by observer S.

The images of the sound of the key were reported also by the fifth observer, A., fluctuating as to distinctness and sometimes vanishing, but present in the majority of cases. Intervals without the auditory image were oftenest judged, Alv. said, 'mechanically,'—the meaning of this is uncertain. A sense of hurry was often experienced in experiments with the long standard. A.'s results were:

Standard:	5	seconds.	Average	error,	—.2
"	10	"	"	"	+2.4
"	3	"	"	"	—.7
"	7	"	"	"	+5.5

Like S. and F., this observer overestimates the short standards and underestimates the long ones. The very large underestimation of the 7 second standard is anomalous.

In spite of the individual differences thus brought out, there is a hint of a general principle to be found here. The two observers, S. and F., who reported the influence of strain and relaxation most frequently, show a tendency to make the longer standards too short in their reproduction of them. On the other hand the two observers Ald. and W., who usually filled the intervals either with imagined movements or with an auditory rhythm, noticing little or no strain, tend to make all the standards too long in their reproduction. Now to measure an interval by the increasing intensity of strain sensations produced as one waits for the end is decidedly fatiguing, on the testimony of the observers themselves, and there is a natural tendency to shorten the longer intervals as a result of this fatigue. On the other hand, when the intervals are mentally filled with a more or less varied content, involving no strain, and very little fatigue, the observer might have easily run a little over the point of objective equality to the standard in her reproduction, especially where, as was the case with W., experiencing the standard interval itself did involve some strain, as she tried each time to impress herself anew with its length, and did not involve the auditory rhythm, which developed only in the long interval. It is too difficult to know what A. means by the 'mechanical' reproduction of the standard to interpret her results with certainty, but the method of measurement by waiting for the recurrence of an auditory image would naturally involve tension and relaxation, and, as we have seen, was explicitly reported to do so by F.

It is clear that speculation regarding the cause of the tendency shown by S., F. and A., to overestimate shorter standards and underestimate larger ones, is of little value owing to the roughness of the method of experimentation. We had some evidence that one influence at work here may have been derived from our procedure of giving a shorter and longer standard in alternation. This influence was the reverse of contrast, the standards tending to be mentally assimilated towards an intermediate value. A. was given a series in which a 9 second standard was used continuously, and this was overestimated, the error being —1.5, although 7 and 10 seconds had been underestimated by the other method. The auditory image played less part in A.'s introspection in this series, most of the judgment being described as mechanical. W., who overestimated everything below 13 seconds, which she underestimated when it was given in alternation with 5 seconds, the error being +2.8, underestimated 14 seconds given without alternation decidedly less, the error being only +.6. Ald., who also lengthened all the intervals but the longest used, 10 seconds, overestimated 8 seconds by an error of —1.05 when it was used in alternation with 4 seconds, but by an error of —2.65 when used alone; that is, there was a certain tendency in the direction of underestimation of this interval when used in alternation with a much shorter one, although the tendency did not amount to actual underestimation. S., on the other hand, who was given the standard of 4 seconds, with which she made the best record where short and long standards were alternated, the error being only —.1, in a series by itself, made an error of only —.2, so that it looked as though she found this interval especially easy to reproduce under any circumstances. F. was not tested on this point.

Rough as the experiments in this study are, their results indicate a few conclusions of some interest.

(1) There is great individual difference in the methods used for the reproduction of the standard interval; the principal ones

are strain and relaxation, imagined movements, auditory rhythm and the spontaneous occurrence of auditory images of the limiting stimulus.

(2) Where strain and relaxation were prominently concerned there was a tendency to shorten the longer standards, possibly on account of fatigue.

(3) Where a short and a long standard were alternately used no contrast effect whatever was visible; rather there were some indications of a tendency to assimilate the two towards an intermediate value.

IV. The Loss of Associative Power in Words After Long Fixation.

By Elizabeth Severance and Margaret Floy Washburn.

The phenomenon referred to in the title of this paper is one that is familiar to most people, but has never, so far as we are aware, been made the subject of experimental study. If a printed word is looked at steadily for some little time, it will be found to take on a curiously strange and foreign aspect. This loss of familiarity in its appearance sometimes makes it look like a word in another language, sometimes proceeds further until the word is a mere collection of letters, and occasionally reaches the extreme where the letters themselves look like meaningless marks on the paper. In the present study we have attempted to observe this process in detail and under experimental control.

To secure uniformity, the words used were all of six letters, printed in long primer type, cut out of the same periodical and placed upon a background of white paper. Words without capital letters were employed. The observers, six women, all with a fair amount of introspective training, were required to look fixedly at a word for a period of three minutes, measured by a stop watch, and to describe all the changes undergone by the appearance of the word. The approximate time of these changes was noticed. A few typical instances of the resultant phenomena are here given:

(1) Word: *career*. Observer: W.
Time: 6 sec. *reer* stands out prominently and gives the word an unfamiliar look.
22 sec. Sound suggestion = care—er.
32 sec. " " = career, for a single instant.
46 sec. The second e looks like c; the sound of the Latin word *carcer* suggested.
1 min. 20 sec. No sound suggestion: the word looks entirely foreign.

2 min. 48 sec. A mere collection of letters, with occasional 'flashes' of the 'career' sound.

(2) Word: *blood*. Observer: S.
Time: 13 sec. Sound blood.
24 sec. " loo, b and d look like each other turned backwards, hence meaningless.
1 min. o's look unfamiliar, staring.
1 " 12 sec. b and d look like p and q upside down.
1 " 35 sec. Sound 'blood,' then 'blōōd,' then 'loo.'
2 " 59 sec. A collection of letters.

(3) Word: *rumble*. Observer: Ald.
2 sec. Sound of thunder.
11 sec. " " bumble bee.
21 sec. Word divides into rum ble.
59 sec. umble. Uriah Heep.
1 min. 21 sec. No meaning to word.
29 sec. u looked like n upside down. No sound or meaning.
2 min. 29 sec. Think of sound, but it no longer fits the look of the word.
2 min. 59 sec. Letters all look queer.

These examples will serve to illustrate the general character of the results. The following fact appeared as the outcome of a careful comparison of the data from all the observers. If the components which enter into the mental complex ordinarily involved in the sight of a printed word be grouped under the three heads of visual, auditory-motor and meaning elements, it may be said that the meaning of the word and its normal auditory-motor image usually disappeared from consciousness within a few seconds after fixation began. The place of the normal auditory-motor image was often taken by others suggested by fragments of the visual word which assumed special prominence; in other cases foreign language sound associations appeared. At instants during the entire experiment the normal sound image might recur 'for a flash.' In the latter part of an experiment all sound and meaning elements would often vanish, yet the combination of visual elements would still be a familiar one. Later, the visual word might lose its familiarity as a combination and be described as "a collection of letters." And finally, even the letters themselves sometimes became unfamiliar, the printed word appearing to be a number of strange marks on the paper. These stages by no means all occurred in every experiment, but they were observed with sufficient frequency to allow their order to be approximately determined as above. In explanation of them the following considerations are offered.

The first effect of gazing fixedly at a word is naturally to concentrate attention upon its *appearance*. Under ordinary circumstances, the look of a word in a familiar language is attended to only for the briefest possible instant; its sound and meaning associations at once enter upon possession of the field of attention. The very fact that the observer in our experiments was told to look fixedly at the word resulted in an abnormal prominence of the visual elements in the ordinary word complex. Several phenomena connected with this fact were observed which greatly contributed to the loss of the normal associations of the word. First, the letters in the word tended to fall into groups. The normal sound association of the whole word accordingly vanished, and each of the two or more groups of letters into which the word disintegrated gave a sound suggestion of its own. Thus 'castle' became 'cast-le,' 'toward' became 'tow-ard,' and so on. In all the observers an inclination was noted to make this division a symmetrical one; 'regret' was 'reg-ret,' 'secret,' 'sec-ret.' Words like 'terror,' 'coffee,' 'yellow,' whose syllables, that is, division of the normal sound image, corresponded to symmetrical divisions of the visual word, kept normal sound and meaning longer than those whose visual and auditory divisions did not coincide; thus 'mother' promptly became 'mot-her.' Secondly, the form of one or two letters in a word would often stand out with special prominence from among the others. These letters were more commonly those with more striking forms, such as 'a' and 's;' 'm' was also a salient letter apparently because of its greater area, so to speak; but the letters b, p, d, q, u and n had a curious way of assuming prominence because of their tendency to reverse themselves. That is, p would look like d upside down, b would look like d reversed and so on. This concentration of attention on a single letter was a powerful factor in bringing about the loss of the normal sound association of the word, and also in destroying its familiar look. Thirdly, when a word was unphonetically spelled, it tended, on being fixated, to suggest the sounds connected with the various letters rather than its proper pronunciation. The word 'caught' for example very quickly called up the sound 'cagt.' This was undoubtedly connected with the tendency of individual letters to become the focus of attention in the visual word. One could not say that the more usual sound association was suggested in place of the less usual one, for the letters 'aught' are never pronounced 'agt;' the fact was that the 'g' usurped attention because of its striking form, and produced its own sound association. On the other hand, the word 'indict' suggested 'in dict' both because the c was attended to and because the syllable 'dict' is more commonly pronounced as in 'diction.' 'Tongue' promptly suggested the sound 'ton gue,' because attention fixed on the look of the word naturally centered as much on the silent letters as on those ordinarily sounded.

Again, the appearance of a word steadily fixated would sometimes suggest the sound association of a foreign language.

Occasionally the whole word would act in this way, as when the word 'circle' called up the Italian pronunciation 'chircle.' At other times when the word had 'separated' into parts, a part would suggest the foreign sound; for instance, in the word 'jungle,' the letters 'jung' associated themselves with their German sound.

The loss of meaning association from the word complex seems to us to be intimately connected with the fluctuations and changes in the sound images suggested. The associative connection between the sound of a word and its meaning is much closer than that between the meaning and the mere look of the word. When, as not infrequently happened in the later stages of an experiment, no sound image whatever was present, although the word as seen retained its familiarity, there was never any vestige of meaning present. The look of a word probably cannot suggest its meaning without the simultaneous presence of an auditory-motor image. The fact that spoken language is both earlier learned and oftener used than written language is a sufficient reason for this.

The meaning, then, vanishes when the proper sound image of the word disappears. But why should the latter go? We have seen that its place is usually taken by other sound images suggested by parts of the visual word, either syllables or letters. The reason why these fragments of the word assume prominence is doubtless connected with the law of shifting attention. The attention, concentrated upon the appearance of the word, cannot remain fixed for more than a few seconds upon this as a whole, but shifts from one syllable to another, or is attracted to a particular letter that has some peculiarity of form; and as now one part of the visual word, now another, is attended to, the sound associations also vary. The normal sound association of the whole word is thus lost.

If we ascribe the loss of meaning and of the normal sound image to the shifting of attention from one part of the visual word to another, how are we to explain the further changes that often occur in the later part of an experiment? At one stage, the printed word, with a perfectly familiar aspect, is attended to as a whole, without the slightest suggestion of either sound or meaning. Then the familiarity mark of the combination may be lost, and the word is a group of letters, the letters themselves being really letters, however, that is, being familiar, though without sound suggestion. And finally, the letters themselves become mere oddly shaped marks on paper. Thus for example, 'pocket' with observer S. first suggested a meaning association, which was lost upon the word's separation of itself into poc-ket; then the c looked like a broken 'o' and the sound suggested was 'pooket;' then the letter k assumed prominence and began to look unfamiliar; finally all the letters except p and o 'looked queer.' The word 'middle,' after suggesting two or three meanings to observer Sea., lost its ordinary meanings through the prominence assumed by 'iddle,' which seemed to the observer to have a vague meaning of its own, as well as a sound; then the word became a mere collection of letters, the d's looking like b's turned backward, and at the end of the three minutes the letters themselves had become 'queer marks.' A fact which interested us about the later stages was that the visual word as a whole could look familiar after it had ceased to suggest either meaning or auditory-motor associations. The same was true of the individual letters. The passage from the stage where the word was a familiar visual object in its entirety to that where it became a mere 'collection of letters' was distinctly later in its occurrence than the loss of definite associations with the word. Similarly, when the letters themselves ceased to be letters and became marks on the paper, it was not because they lost at that moment all suggestion of sound; the sound had usually gone sometime before. It was a loss of familiarity, not of association; and the experiments illustrated very clearly the fact that familiarity, recognition, does not necessarily involve associated ideas.

But why do the shifting associations disappear, and why does the familiarity itself ultimately disappear? If the shifting associations are due to the tendency of attention to fluctuate, the same tendency which is illustrated in rivalry, and is here shown by the wandering from one part of the visual word to another, why does not this process keep up indefinitely? It looks very much as though we had to deal here with something like auto-hypnosis. Prolonged concentration of the attention upon an object which, while it does not remain entirely unchanged, yet can change only within narrow limits and with constant recurrence to the same phases, produces an increasing narrowness of the field of consciousness; the associations drop off entirely, then the 'fringe' of familiarity goes, and finally we approach as nearly to a bare, peripherally excited sense impression of the marks which normally are letters, as we can get in adult experience. Whatever may ultimately turn out to be the explanation of the narrowing of consciousness in hypnosis brought on by attention to a monotonous object, will explain the final loss of associative power and of familiarity in a word long fixated. For attention, pure Being and Not-Being are identical; we attempt to reach perfect concentration of attention upon a single object, and the object dissolves in the process.

In conclusion we may add that analogous phenomena have been observed by many people as occurring when a spoken word is repeated a number of times, so that attention is abnormally concentrated on its sound. Experiments upon this oint are to be undertaken in the near future.

ON SYSTEMATIC ERRORS IN TIME ESTIMATION.

By F. M. URBAN, University of Pennsylvania.

It was found in a statistical study on the estimation of time-intervals[1] of different length and of different filling by a great number of subjects that the numerals (0, 1, ... 9) did not occur with the same frequency at the last place. The numerals zero and five occurred with the greatest frequency and the numbers next to zero and five (1 and 9, 4 and 6) occurred with the smallest frequency. Since there was in the statistical data no indication for a further analysis of this fact, it seemed necessary to go back to the study of this phenomenon in individual psychology. An investigation was about to be undertaken when a paper of Mr. O. Meissner[2] was published, which discussed the estimation of short time intervals in terms of fractions (tenths) of a second. These results show striking similarities with ours but they show also some differences and considerable light is thrown on the nature of the mental process of estimating time by comparing both results. I owe to the kindness of Mr. Meissner some important information and valuable help which assisted me greatly in this work.

Mr. Meissner's results refer to the estimation of short time intervals by trained subjects. The results are taken from the observations of transits of stars made by three observers, who are called W, K, and N. N made 12,285 observations; K and N made 8,505 and 16,215 observations respectively. The series of observations was extended over several years and with two of the observers intermissions of considerable length occurred. At the beginning of the series N was 35 years, and W and K were between 20 and 22 years. N was a practiced observer; the practice of W and K was not great but they had had some practice. In so great a number of observations one would suppose that every tenth should occur in the judgments approximately with the same frequency, *i. e.*, in 10% of all the cases. The results, however, show that certain numerals are always favored, *i. e.*, they occur in a higher frequency than 10%. This predilection is subjected to variations, which may be considerable and which are apparently quite irregular. This preference for certain numerals constitutes a systematic error which one cannot obviate by combining the results, because only accidental errors can be eliminated in this way. These systematic errors varied much with W, in whose judgments the frequency of zero in the last part of the series was one-half of that in the first part, but they remained fairly constant with N. The fact that N was a practiced observer and that W had had not much practice suggests the view that these variations depend on the practice of the observer.[1]

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[1] In a joint publication of the author with Mr. R. M. Yerkes, Time Estimation in its Relation to Sex, Age, and the Physiological Rhythms, 1906, Harvard Psychological Studies, Vol II, pp. 405-430.

[2] Otto Meissner: Ueber systematische Fehler bei Zeit und Raum Schaetzungen, Astronomische Nachrichten, Aug., 1906, Vol. 172, No. 4113. A short report may be found in Sitzungsberichte der Berliner Mathematischen Gesellschaft, 25, April, 1906, pp. 70-72 (Archiv der Mathematik und Physik, Vol. X, Nos. 3 and 4).

TABLE I.

Frequency of the Numerals (0 to 9)—W 12,285 Observations, K 8,505 Observations, N 16,215 Observations.

	W	K	N	Average.
0	9.04	21.59	19.23	16.62
1	9.45	3.89	5.24	6.19
2	12.32	4.72	12.49	9.84
3	11.15	9.35	12.52	11.02
4	11.97	15.10	10.91	12.66
5	6.35	11.17	8.09	8.54
6	8.28	10.39	4.45	7.71
7	11.63	9.00	5.19	8.61
8	11.37	7.04	14.37	10.93
9	8.45	7.75	7.52	7.91

The frequencies of all the numerals are given in Table I. The first three columns under the headings W, K and N, give the individual results of these observers and in the last column these results are combined into an arithmetical mean. In the individual results the great personal differences are obvious at once; the frequency of 2, for instance, varies from 2.49% to 12.32%. The general trend of these results becomes clearer by an examination of their average. They show (1), that the small numbers (0 to 4) occur more frequently than the high numbers, the sum of the percentages being for the low numbers 56.33, and for the high numbers 43.70. The sum of these percentages is not exactly 100 owing to an error of computation. (2). The numbers do not occur with equal frequency, zero having the highest frequency, 3 and 8 occurring next with a small difference which might be accounted for by an inaccuracy of the numerical determination. The smallest frequencies are those of 1, 6 and 9 in the order given.

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[1] See Meissner: l. c. Tables I, II and III.

For a comparison of Mr. Meissner's results with ours Table III (Males) of the article quoted above comes into consideration. These numbers are not directly comparable with Mr. Meissner's results because the estimation of long time intervals is not only under the influence of the preference for certain numerals but also under the influence of the conventional minute standard. The numbers favored by the latter influence are the simple fractions of a minute (15″, 30″, 45″ and 60″) and their multiples. The final digit of such numbers is 0 or 5, the frequency of which is increased by this influence, which must be eliminated for the comparison because the results of the estimation of short time intervals are free from it. This can be done in this way. Among all the judgments given there are 1,666, the final digit of which is zero. According to chance one-third of these (555) should be multiples of 30, because among three consecutive multiples of 10 there is one multiple of 30. As a matter of fact there are 732 and by subtracting this surplus of 207 the influence of the minute standard may be eliminated. A similar computation may be made for the numeral 5. On the basis of this computation one may correct the relative frequencies of the other numerals; the results are given in Table II.

TABLE II.

Corrected Frequency of the Numerals 0 to 9.

	Frequency.	Percentage.	True Value.
0	1177	36.724	
1	121	3.775	0.202
2	217	6.771	0.255
3	155	4.836	0.313
4	144	4.493	0.360
5	660	20.593	0.485
6	153	4.774	0.612
7	170	5.304	0.663
8	281	8.767	0.733
9	127	3.963	0.796
	3205	100.000	

A comparison of these results with the numbers of Table III of the previous article will show that this change does not affect the relative frequencies in a material way. The most favored numeral in this case, too, is zero, the next is 5 and then comes 8. The numerals 1, 9, 4 and 6 have the smallest frequency in the order given. These results agree with Mr. Meissner's observations by giving the greatest frequency to 0 and by showing that 8 is favored and that 1, 9 and 6 are at a disadvantage. They do not agree in regard to the frequency of

5 and 4. The most striking difference is that of the numerical values of these relative frequencies. This difference is due to the overwhelming frequency of 0 and 5 which affects the frequency of the other numerals in such a way that they are throughout smaller than those of the observations of Mr. Meissner. The influence in favor of 0 and 5 is much stronger in our experiments.

This partial verification of these two series of experiments might be interpreted as indicating a common factor in both series of observations, the influence of which was partially counterbalanced by some other circumstances. The common factor may be called the preference for certain numerals and the nature of the other influence can be studied by means of the following considerations.

Among the results of W, there are 9.04% cases in which he observed the interval 0 and 9.45% cases in which he observed the interval 0.1.″ This means that all these intervals which differ by not more than 0.0452″ from zero—*i. e.*, the interval from—0.9548″ to +0.0452″—are judged to be zero. Correspondingly the end of the interval which he considers to be 0.1″ has the distance from zero $0.045 + \frac{1}{2}.0.94 = 0.045 + 0.047 = 0.092''$. This means that all the intervals from 0.046″ to 0.137″ are judged to be 0.1″, and 0.092″, the average may be regarded as the interval corresponding to W's estimation of 0.1″. By continuing this operation one may find for all the other numerals the corresponding intervals which are called the true values of the estimation. They are given in Table III for Mr. Meissner's observations[1] and in Table II under the heading "True Value" for our experiments. Table III shows that the estimated intervals coincide most closely with the actual intervals for 0.6″ and 0.7″, and that W has the tendency to overestimate and K to underestimate. The judgments of N show

TABLE III.

	W	K	N
0.0	0.000	0.000	0.000
0.1	0.092	0.127	0.122
0.2	0.202	0.171	0.211
0.3	0.319	0.241	0.337
0.4	0.434	0.364	0.404
0.5	0.527	0.495	0.548
0.6	0.599	0.603	0.611
0.7	0.699	0.700	0.659
0.8	0.814	0.780	0.757
0.9	0.913	0.849	0.867

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[1] See Meissner's l. c. p. 141, 142.

a constant overestimation for the intervals from 0.1″ to 0.6″ and from this point the overestimation turns into an underestimation, the amount of error being smallest and the accuracy being greatest for 0.6.″ The judgments of W and K bear out the same relation: the exactitude is greatest for 0.6″ and 0.7.″

These figures agree very well with the results of previous psychological investigations. Small time intervals are, as a rule, overestimated and larger time intervals are underestimated. There lies between the large and the small intervals a point of indifference at which the estimated length of the intervals correspond most accurately to the real length. This point lies, according to different observers, between 0.5″ and 0.7″ and uniting these results into a mean one may take 0.6″ for this point.[1] These conditions are exactly verified in the observer N who had many years practice in these observations. The two other observers show slight differences. W overestimates small intervals and is most exact in his estimations of intervals of 0.6″ and 0.7″ but he overestimates also the intervals of 0.8″ and 0.9″. K underestimates long and short intervals but also his estimation is most correct for 0.6″ and 0.7″. These facts account for the great frequency of the small numerals.

The great frequency of zero, too, can be explained from the conditions of the experiments. The judgment zero will be given if the star crosses the line in the moment of a beat of the clock. From the experiments with the "Complicationsuhr" we know that for the most part the sound is located at a point which is distinguished from the other points by some marks. Thus it occurs but seldom that the sound is located between the dashes of the clock.[2] The lines in the telescope play the same rôle as the marks on the dial of the clock and, therefore, the beat of the clock is heard more frequently at the moment of the star crossing the line. This accounts not only for the excessive frequency of zero but also for the low frequency of 1 and 9, because if in a series of equal small time intervals one interval is marked in such a way as to induce an overestimation of it, then the overestimation is made at the expense of the adjoining intervals which are underestimated.

It is obvious that a similar computation of the "true values" for our results cannot have the same signification. The results, indeed, show that the estimation comes nearest to the true value for 0.6″, but the change from overestimation to underestimation is quite irregular. The intervals from 0.1″ to 0.3″ are overestimated, then this overestimation changes into underestimation for 0.4″ and 0.5″. The interval of 0.6″ is overestimated again but the other intervals from 0.7″ to 0.9″ are underestimated. This warrants our statement that the factor, the influence of which on the judgment of short intervals was just proved, is not at work in the estimation of large intervals. The explanation of the very high frequency of zero and five in the estimation of long intervals is to be found merely in the general inaccuracy of the estimation which is expressed by the preference for round numbers.[1] The low frequency of the numbers which are next to the round numbers may be explained by the subject feeling the inaccuracy of the judgment which does not warrant a deviation from a round number by so small a margin. The view that the low frequency of these numerals is due to underestimation of the seconds just preceding or following every fifth and tenth second has but little psychological probability. It is contradicted also by introspection which shows that the judgment in this case is not based on the perception of a succession of small intervals among which, perhaps, every fifth or tenth is accentuated, but the estimation refers to the vague notion of intervals of approximately the same duration. It does not seem possible to give at present a reason for the differences in the frequencies of the other number (2, 3, 8 and 9), among which the even numbers occur with a higher frequency than the uneven. An explanation is not offered by saying that the different frequencies of the numerals are due to a preference for certain numerals, because, without introspective or objective evidence for such a predilection, the only possibility of giving an account for this preference is to refer to the different frequencies. "Preference for certain numerals" and "different frequencies of certain numerals" are only different names for the same thing and the first term is misleading in so far as it might lead one to suppose that there exists a certain mental process which in every case is the cause of the differences in the frequencies of the numerals. There is positive evidence that such a process does not exist. If there existed such a mysterious process then it could not happen that the same numeral occurs with a frequency which is subjected to variations which are so large as those of the frequency of 4 and 5 in our experiments and in those of Mr. Meissner's series. It seems reasonable to take the view that the terms "preference for certain numerals" is a short designation for an unanalyzed group of conditions which is the cause of the differences in the frequencies of the numerals; these groups of conditions may be widely, or perhaps even absolutely, different in two series of experiments.

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[1] Wundt, Physiologische Psychologie, 5. ed. Vol. III, p. 47. Wundt refers to the investigations of Kollert, Estel and Meumann; a thorough discussion of their results is to be found in I. Quandt, Das Problem des Zeitbewusstseins, Litteraturbericht, Arch. f. d. ges. Psychologie, 1906, Vol. VIII, pp. 143-189.

[2] Compare Wundt: Physiologie Psychologie, 5 ed. Vol. III, p. 69.

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[1] Other reasons for this statement may be found in the article on time estimation, pp. 411-418.

The comparison of these two series of observations shows that there are certain common results (high frequency of certain numerals, low frequency of the numerals adjoining to them). The high frequencies are apparently due to different mental conditions, but the low frequencies of the adjoining numerals may be brought under the general rule that the elements of a complex which are next to a favored complex with which they are not associated are at a disadvantage. The chief result is that the so-called preference for certain numerals is a complicated mental process which admits of a further psychological analysis. The analysis of these two series of observations shows that this preference for certain numerals may be very similar in its numerical expression and yet it may be due to entirely different conditions.

STUDIES IN ABSOLUTE PITCH.

By Lucinda Pearl Boggs, Ph. D.

Absolute pitch is one of those curious psychological phenomena which is at present rather a puzzle. Its rarity makes it difficult of investigation and usually the people who possess it are not trained in psychological introspection. However, it has been my good fortune to know a number of people who possessed this remarkable gift and to have been able in some cases to carry out a few tests which seem to me worth recording. The fact that they were only casual acquaintances and not students of psychology, and that they were far away from a properly equipped laboratory, precluded the elaborate and extensive tests which it might be desirable to make.

By absolute pitch we mean the power to recognize a single musical note when heard, without comparison with any other tone, either objective or subjective. Von Kries[1] and Stumpf[2] both say that this independence of other tones is the essential characteristic of absolute memory for tones, as they have found in the process of their experimentation. The latter admits that at one time he believed the recognition of tones was dependent on the judgment of intervals, since the observer could not forget the previous tone or the indispensable *a* of the musician, but found by a series of tests that only exceptionally did the recognition of the interval affect the judgment. Whipple[3] also finds that the interval sense is not used at all and that his subject is very hazy in her designation of intervals. The fact upon which all psychologists are agreed, that the more quickly the judgment of the tone is made, the more accurate it is, also goes to show that intervals are not taken into consideration.

As to the factors which contribute toward the accuracy of absolute memory for tones, the timbre of the instrument and concentration of attention have been shown to be the most important. Von Kries[4] even considers that the slight not immediately discernible differences in the timbre of tones differently struck on the same instrument might influence the judgment. The tuning of the instrument is also likely to influence the judgment if difference in the tuning is very great, though it has not been found that people with absolute memory have a finer pitch discrimination than other musical people.

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[1] Von Kries: Uber das Absolute Gehör, Zeitschrift für d. Psychol. u. Physiol d. Sinnesorg., Vol. III, p. 265.

[2] Stumpf: Tonpsychologie, Vol. I, p. 306 ff.

[3] Whipple: Studies in Pitch Discrimination, *Amer. Jour. of Psychol.*, Vol. XIV, p. 292.

[4] Von Kries: *op. cit.*, p. 271.

The question whether absolute pitch can be acquired by practice or whether it is a "free gift" of heredity is one upon which it is hoped this paper may throw some light. Von Kries[1] says that he has never found any one who had genuine absolute pitch through practice, while M. Meyer[2] has shown that a fair degree of absolute memory may be acquired. Stumpf[3] attributes at least something to practice, as he has noticed that different people judge correctly oftenest in those tone regions with which they are most familiar. Hence he finds that with most persons judgments are oftenest correct in the middle region of tones, which are, of course, the ones oftenest heard.

As to the nature of this "gift" of absolute tone memory, as Stumpf[4] says, it must manifestly be placed under memory in general. He thinks, with Bain and Ribot, that the fineness of the sensations may have something to do with the richness of the memory. "One might say: when a sense organ presents finely gradated sensations, each one has then more help in reproduction. But on the other hand, one might equally as well conclude that the great number mutually confuse one another and disturb the reproduction. Better say, perhaps, that through the richer material a stronger necessity, and through the fineness of the possible distinction, a greater stimulus arises for employment and practice with this sense. Retentiveness itself is not greater, but the number of cases of practice increases through the variety of distinguishable sensations." In the next paragraph he says that "the completeness of the memory is more closely connected with the liveliness of the feelings than the fineness of the senses, in so far as the feelings give rise to a constant direction of the attention to a certain field of perceptions." The conclusion drawn from my own experiments agree as to the effect of a very sensitive musical hearing and the liveliness of the feelings connected with tones, as I shall show later.

The first subject in the following experiments was Miss G. G. Gulick who is extremely musical and comes of a very musical family on her mother's side. Her mother was a pianist with considerable power of improvisation and possessed a remarkably sweet contralto voice. Her maternal grandmother and great aunt were extremely musical. Her older brother was a piano virtuoso and a composer, and her sister, though not a musician, was fond of music for "the thrill it gave her." She thought her mother had absolute pitch, and was quite confident that her brother must have possessed it, but as both of them had recently died, there was no possibility of verifying the supposition. Her musical education began at about the age of four with her mother and was continued under governesses until she was thirteen. From that time on, she had lessons from professors of music good, bad, and indifferent. Three months comprised her work in the theory of music, and a four years course in solfeggio covered in a few months, her vocal training. She composed little songs for children and sometimes improvised. She had had no training in memorizing tones, but always, even as a child, had been able to recognize the notes of any instrument such as a bugle, saying that it played *mi* or *la*. To test this, I once asked her the note of the dinner gong. She instantly pitched the tone with her voice, saying, "it is sol." Tunes heard on the street were easily memorized by her as a child, and reproduced on the piano. Though very faulty in technique, the result of her unsystematic training, she had the power of interpretation of music, and could give each tone a certain value in itself.

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[1] Von Kries: *op. cit.*, p. 262.

[2] Meyer: Psychol. Rev., VI, p. 514.

[3] Stumpf: *op. cit.*, p. 311 ff.

[4] Stumpf: *op. cit.*, p. 287 ff.

She had never heard of absolute tone memory, and when questioned as to whether she could name any note heard on the piano, was very uncertain and later was quite astonished to find that she could do it, almost without error. She had no muscular or visual sensations in judging the tone, it seemed entirely due to the sound itself. With the single tones she had no musical imagery as she had with phrases and strains. High, lively notes were bright yellow; low tones were purple or dark red; middle tones were green; when low tones were purple and high notes were added, they merged into light red and yellow. Some music seemed to her opaque, other music was like the rainbow. Likewise she professed that single notes had no emotional tone or feeling, though music often made her cry, even the gay sort. She could hear overtones so plainly, she said, that she had difficulty in picking out the fundamental tone.

This observer was entirely without practice in introspection. At first she said she calculated each tone from the preceding one, though she "pitched in on the first, right or wrong." A little later during the same test she said that her judgment was "instinctive," without any idea of the relations of the tones, and that all was due to the sound itself. Several times she asked for a note to be repeated in order to fix it firmly in her mind and would sometimes name the note correctly on the second or even third trial. Her replies were usually given very quickly and with confidence. She was of a very decidedly objective type of observer, concentrated her mind well but was easily thrown out by any noise or confusion.

The table is easily understood, the numbers under *S* giving the series of tests; those under *O* giving the number of octaves from which the notes were selected; those under *T* the number of tones used in each series; under *R* is given the number of tones recognized correctly; under *W* the number of errors and under *P* the per cent. of right cases. The letters G, C, D, and E refer to the observers. The tones were selected at random within a given number of octaves, and there seemed to be little or no difference in the tone region, so that I have not made a particular rubric for that. The piano generally used was not especially good, though the best available at the time. The experiments were carried on in a quiet room without interruptions.

G					
S	O	T	R	W	P
1	3	40	36	4	90
2	2	40	33	7	82½
3	5	50	50	0	100
4	4	50	42	8	84
5	3	25	23	2	92
6	3	10	7	3	70
7	3	25	22	3	88
8	1	10	7	3	70
9	4	25	4	21	16
D					
10	4	50	34	16	68
C					
11	2	10	10	0	100
12	4	50	29	21	58
E					
13	5	50	46	4	92

Series I. Four errors, all minor seconds, were made, each one being a sharp, or black key. This peculiarity was most striking and in her subsequent tests it was also found that nearly all her errors were on black keys. When told of this she was much surprised and was at a loss to account for it. Finally she explained it as a result of her having always, until recently, called notes by syllables of the tonic *Sol-fa* system and had not called the black keys by any name different from the white keys, as *A* sharp, *E* flat, etc. Twice she was dissatisfied with her judgment, and on having the tone again given made a correct judgment. In these cases the error amounted to a major third and a minor fifth. Two chords given were also judged incorrectly the first time, but repeated at her request were judged correctly.

Series II. Only seven black keys were given and five of the seven errors were made on those tones, while the other two errors followed these and were perhaps influenced by the preceding errors as a suspicion of a mistake was likely to disturb the observer.

Series III. No black keys were given in these tests which were made on a strange, but very good new piano. This series brings out clearly the fact which the other series indicated, namely, that the tonal region made no difference with this observer. Where black keys were unevenly distributed in octaves, it was not possible to reach this conclusion satisfactorily.

Series IV. All but three errors are half tone errors on black keys. There were sixteen black keys struck and only five are judged incorrectly, which is a considerable improvement over Series II, where five out of seven are incorrectly judged. That this is due more to carefulness in naming tones than to practice in discrimination, is my opinion. The other errors are a major second, a minor second, and a major fourth. The last error was due to distraction, the subject reported.

Series V, without any black keys on the piano generally used. The errors were both half tones and both cases she reported a wandering of attention.

Series VI was a test on a clarionet. In reality twenty-five tests were undertaken, but at the close of the first ten, there was such a disturbance in the next room that only fourteen of the remaining fifteen were correctly judged, although in no case was the error more than a whole tone.

Series VII was a test on a violin. Both errors consisted in calling *f. g*. No sharps were introduced. The violin was a strange one, but the observer was familiar with violin music.

Series VIII was a test with the voice. The three errors were made on sharps, two being minor seconds and one a major second.

Series IX is a test which can hardly be tabulated with the others. Instead of single tones, chords of from two to four notes were given. All of the two tone chords were correctly recognized, but only one three note chord. The upper note of each chord was recognized twenty times out of twenty-five, but was omitted in four tone chords. The lower tone was right fourteen times in three tone chords and omitted twice in chords of four tones. The middle notes were recognized only seven times and were omitted three times. Estimating the upper notes, her per cent. of error was 20%, larger than in any other piano test, while the per cent. for all notes in the chord is 55%. Here, as elsewhere, the black notes were the ones most often miscalled. I was not able to make all the notes stand out distinctly in the chords as in single tones, which may have influenced the results.

Considering Miss G.'s number of errors on the half tones, it would have been very interesting to test her ability to detect fine tonal differences. Unfortunately the time of her experimentation was cut short by her departure, but the following crude tests were made. The gamut was played chromatically on the violin with quarter instead of half tones. She thought it was the regular chromatic scale. Again approximately one-eighth tonal differences were given on an *E* violin string, the observer being informed of the object of the test, and she was required to judge which was the higher of two tones. Her judgment was correct in about half the trials. This shows that her pitch discrimination was not fine.

The second subject was a young woman, Miss C. Dismukes. At the time I met her she was recovering from a long nervous illness, so that extended tests were impracticable. Her musical education began so early that she could not remember when or how she learned to read notes, but at six she was playing in concerts a little. None of her immediate family are musicians, but her maternal grandparents were very musical and of an artistic temperament. At ten years of age she began the serious study of music, and studied one year in Chicago at the age of nineteen, where she had a little harmony; she discovered in that work that she could write down simple melodies, tone for tone, as she heard them. This was followed by three years in Vienna with Leschetizky, who tested her very superficially as to her memory for tones, but she had never practiced memorizing tones at all.

She thought this tone memory was a help to her in memorizing music so readily, and she always played in concerts without notes. She improvised and composed simple melodies to words. Besides concert playing she gave lessons in music. She sang a great deal for her own amusement, and found that she could recognize tones from the human voice more easily than from any instrument. Of the tones of instruments, the piano, her instrument, were the most readily recognized. She also reported that she could hear overtones very plainly. Series X was made on a fairly good piano, often used by the observer. Nearly half her errors was in the lower tones, and a larger proportion of the remaining was in the upper tones. Black keys were recognized as easily as the white keys. Although she gave all of her answers rather slowly, often humming the tone, to fix it in her mind, she said, she believed that her recognition of the tones was immediate, *i. e.*, not calculated from any other tone. A correct judgment came more quickly than a doubtful one. An octave and a major chord were easily recognized. This observer found it difficult to concentrate her attention, and if she suspicioned that she had made an error, she was disturbed and was likely to make other errors. Half the errors were minor seconds and two minor fourths were the largest errors.

The third subject, Solomon Cohen, was a boy of fourteen who had absolute pitch. He was a student of the violin and had studied since he was eight years old. He sang a great deal even at four. The whole family was musical, and he had heard music of every sort ever since he was born. He thought that *f* was the easiest note to recognize and the scale of *f* was the one which he liked best and in which he played the most. His musical brother's favorite scale was *C*. They had never had any training along the line of memorizing tones, but had been taught to detect inaccuracies in the pitch of any instrument, which they did easily. *S* had a very keen ear for overtones, I discovered in testing him, and he could name those he heard most readily. His introspection in regard to naming tones was that he "just knew them and had no other note in mind."

Series XI. In the short series on his own piano, S. made no errors, although black keys and one chord were introduced. The octaves were the large and small octaves. His judgments, although given rather slowly, were not marked by hesitation.

Series XII. This series was given on a piano strange to S and rather out of tune. He remarked that the tones did n't sound natural. This experiment has value chiefly as showing the extreme effect of distraction. Another very musical boy of the same age was trying to name the tones. If S named the tone first he was almost sure to be right, while if the other gave his judgment first, S either agreed with him or was likely to make an error. The other boy did not make half a dozen independent judgments which were right. S said that black keys had a different sound from white keys, and though he made some errors on the black keys, he never misjudged a black key for a white key or *vice versa*. He could not explain the difference; he said, "You just know it."

The fourth subject was David Elliot, aged eight years, and was reported by his instructor in music who thoroughly under-stands the subject of absolute pitch from the musician's standpoint. The boy began to study music at the age of six, and while his family are not musical, there is a strong artistic strain in his mother's family. He memorizes music readily, and is very clever at writing music. The piano is his instrument, but he recognizes tones of other instruments and the human voice, and he can tell readily when a tone is not true. So far as the teacher can ascertain, he has no visual or motor imagery when listening to tones, nor does he judge from the preceding note. When asked how he knew he was right, he answered, "I don't know that I'm right—I *think* I'm right." This case is particularly instructive because of the early age of the subject, and because his first teacher discovered this ability and can say that he has had no special training along that line. In the test given, series XIII, two errors are major seconds, one a minor third and one a minor fourth.

Some two or three years previous to my work with these observers, I made some tests with Miss Meyer, whom I by chance discovered had absolute pitch, but owing to a press of work and an abrupt departure, I did very little systematic work and kept no records, therefore I beg to refer any interested reader to Whipple's[1] article, whose attention I called to this case. Her per cent. of error, according to Whipple, varies from 64% to 92%, which is not so accurate as some of my subjects, and there is one fact which I do not find mentioned, namely, her very keen hearing for overtones, which was to me very remarkable.

In marked contrast with the method of these persons was that of a Miss J., who tried a series of twenty tests. Only one tone was judged wrongly and that the first one, the error being a minor third; nine were right on the first trial, six on the second, two on the third and one on the fourth and fifth each. Before making her judgment she would hum the note heard and then run over several notes with her voice until she was reasonably sure. If she failed to get it satisfactorily, she would ask for the note to be repeated and would then go through the same process until satisfied. Her introspection was that her judgment was influenced by the preceding tones. Though musical, Miss J. did not call herself a musician, and thought she had learned this way of recognizing tones from her class lessons in singing at school. Another observer in a memory experiment showed a particularly good memory for tones. Although a violinist of considerable skill, he attributed it largely to the practice in his class singing lessons of finding one tone from another, *e. g.*, taking *do* from the pitch pipe, finding

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[1] Whipple: *op. cit.*

fa, so that on hearing a tone he tried to locate and name it, and in this way it became fixed in his memory. Another case of good relative pitch was that of a musical young woman who placed the first note a major second too high. The succeeding half-dozen notes were located correctly relative to the first. The next test, she placed the tone one-half note too high and located the other according to that quite correctly. She "only guessed at them" was her introspection, which is never the introspection in the case of absolute pitch.

In attempting a summary of the different cases reported, there are found to be some characteristics common to all, and again, certain peculiarities of each. Naturally we are inclined to consider the common characteristics as having the greatest bearing on the problem of the nature of the act of recognizing tones from memory. Our conclusions are as follows:

1. All of the observers have had a musical inheritance and an early musical environment and training, but in no case has there been specific training in memorizing tones.

2. An immediateness of recognition and a sureness of judgment is to be observed in all when the judgment is correct.

3. All observers have had a particularly good hearing for overtones, so far as reported. Their pitch discrimination has not been reported to be extraordinarily fine.

4. Concentration of attention was necessary to all, and noises or interruptions were disconcerting and influenced the accuracy of the observer. Each note had to be very distinctly heard.

The first two characteristics seem to indicate some special endowment of the individual who has absolute tone memory, and the last two point to its being some special fineness in the sense of hearing, probably that for distinguishing overtones so distinctly.

The remaining conclusions apply only to certain observers, and are the following:

6. The region of the tone affects the accuracy of some and not of others.

7. Although there was a rather strong emotional accompaniment for music, there was not for single notes except in one case.

8. In one case only did the semitones or black keys prove more difficult of recognition. Then it was perhaps to be attributed to a confusion of names rather than confusion of sound. In all other cases the semitones seemed to be as distinctly associated with a name as the whole tones.

9. In one case there was visual-motor imagery; one liked to fix a tone by humming it; the others did not report any help of this kind.

10. Under the most favorable condition two observers made no errors, while the per cent. of right cases with the others varied from 92% to 68%. The method of recognizing was, however, substantially the same.

These last peculiar characteristics are interesting and agree in part with those discovered by the authors already quoted, as do also the first four reported as common to all. Other investigators might, of course, find other than these four factors common to all their observers and find these as only peculiar to some.

Of course, aside from the purely scientific aspect of the matter, there is also a pedagogical interest. Even though these individuals have some particular ability, could other people imitate their method? For example: supposing that fineness of hearing for overtones is the *sine qua non* of absolute pitch, would a cultivation of the hearing of overtones aid in acquiring a fair degree of absolute pitch? The invariable associating of certain tones and certain names is of the greatest importance, as anything which has a name of its own has a more certain place in consciousness.

This would mean that in singing lessons the tones should always be named and should always be accurately pitched. It is the earnest hope of the writer that psychologists or other persons interested in this subject will investigate and report any cases of absolute memory for tones, as it is through studying mental peculiarities that we sometimes arrive at valuable results in the study of the laws of consciousness.

Studies in Absolute Pitch. Part II.

Since sending in the MS. of the above it has been my good fortune to interview nine persons and to have had letters from four people who seem to have absolute pitch according to the definition of it given above. They have included one of the greatest, if not the greatest, violinist of to-day, and young students of music who are very deficient in their musical education. The degree of ability to recognize tones has varied correspondingly from persons who never make a mistake to those who cannot recognize black keys with any certainty at all.

Their introspections may be summarized as follows, and where the entire thirteen have not agreed it is because of lack of information unless otherwise specified:

1. All report that they have never had any training in recognizing tones, and consider it a natural or "inherited" ability.

2. All report a musical inheritance and a more or less musical environment, as well as an early musical education.

3. Eleven report hearing overtones very plainly, and I have

personally tested the less experienced musicians. Two have not reported on this by letter.

4. An immediateness of recognition without comparison with other tones or without motor-visual imagery of any sort is reported by eleven. Each note has a sound quite distinctive and irrespective of its place in the scale.

5. Seven report that they must be able to concentrate their mind, *i. e.*, must not be over fatigued or disturbed to attain the best results. This I have found to be true in all whom I have tested.

6. The two most unpracticed observers could not recognize the black keys, and were quite confused when they were sounded. However, one of these judged very accurately on the white keys.

7. Seven report that they can pitch any note with their voice. One lady cannot judge the notes of a voice unaccompanied by an instrument.

8. Five report that changing from a piano tuned a little lower or higher makes practically no difference in their judgments.

9. Nine report never making an error in judging a tone. These are all teachers and concert players. The others have varying ability.

The clue to the difference in the method of those having relative pitch and those absolute pitch seems to me to be found in the two facts of hearing overtones so plainly and of hearing each note quite distinctly from its relation to other tones. At first I was inclined to attribute the difference in sound to the keen hearing of overtones, since we recognize different instruments from the variation in their strong partials, and from the fact that a chord is more easily recognized than a single tone. While this may be a valuable aid, further study inclines me to the belief that there may be a tone system of qualities comparable to the spectrum in vision, and is probably due to difference in vibration rates. The scale of tones would correspond to variation in brightness, which is, of course, to be observed in the spectrum as well as the colors, and probably the average person hears the tone system as a tone scale in much the same way that a color blind person sees the spectrum as a graduated succession of brightnesses. I asked a New England Conservatory graduate who has absolute pitch whether she had the tones in her mind as she did colors. Her answer was that she had a tone system as she did a color system. The famous violinist thought that people who did not have absolute pitch must be tone deaf to a certain extent. Neither carry one tone memorized to which they refer all others.

I have never yet found any one who thought that hearing overtones was a help in recognizing a tone, on the contrary they say it is in the tone itself, but it seems that if it lies in the tone itself, the proper way to teach young beginners would be to have them attend to the tone and its overtones so that they may know it for itself, instead of in its relation to other tones. Although very unskillful at recognizing tones myself, I have made greater progress since I have been hearing out overtones and trying to distinguish differences in the tones aside from their relation to other tones.

The following extract from Percy Goetschius Mus. Doc. puts the problem and its probable solution very neatly.

''Some music-lovers can distinguish these varying rates of velocity so accurately that they know which tone is sounding, without reference to the keyboard or comparison with other, previously defined tones. Not all persons, however,—in fact, not many,—possess this ability of defining the absolute pitch of tones; and it might therefore be concluded that any individuality of key that is based upon this distinction would be recognizable only by the very small minority of music-lovers who are thus able to define the pitch of a tone. But that proves nothing; the distinction does surely exist, whether few or many are immediately conscious of it and able to define it.''

EFFECT OF CHANGES IN THE TIME VARIABLES IN MEMORIZING, TOGETHER WITH SOME DISCUSSION OF THE TECHNIQUE OF MEMORY EXPERIMENTATION.

By PROFESSOR JOHN A. BERGSTRÖM, Indiana University.

(With the co-operation of Supt. W. H. Sanders and Supt. F. A. Herrington.)

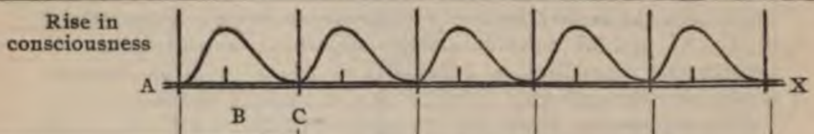
The effect of changes in the different rate variables in memorizing has been the subject for occasional investigation in the Psychological Laboratory of Indiana University since 1894-5, beginning at that time with the preparation of a Master's thesis by Supt. Sanders on the effect of different rates of reading lists of associable and dyssociable words and letters upon their retention and reproduction by auditors. The following year, with a view to extending the investigation to impressions received through the eye, and with a view also to securing apparatus of general utility for memory experiments, the writer designed the compound interrupter and exposure drum, figured and described further on. With this apparatus several studies were begun at different times, but none completed till Supt. Herrington made use of it in the preparation of a Master's thesis, in 1903-4, dealing with the effect of varying, in the learning of nonsense syllables, the duration of exposure of each syllable, the intervals between syllables, and the intermission between readings of series of syllables.

The points of special interest in the investigation have been partly technical and psychological and partly educational. The study of the rate variables in memorizing is, of course, a direct study of certain elements in the technique of experimentation in this field. To avoid the possible disturbing influence of these elements, experimenters have usually endeavored to keep them constant throughout an investigation; the study of their effects should reveal the significance of variations in them and also under what conditions the best experimental results may be expected. The apparatus required must not simply be serviceable for ordinary experimentation, but must, in addition, be adjustable with regard to the variables to be studied, so that what will serve for this purpose will serve for most, if not all, others in which comparable materials and methods are employed.

With regard to the psychological aspects of the problem, it may be said to be a study, on the one hand, of the degree of possible adjustment or adaptation to the different rate variables; and, on the other hand, to be a study of the various mental effects of changes in these variables. The wide range of possible adjustment of the mental processes involved, and the individual differences, are as remarkable in this as in most other lines. But adaptation is not complete and uniform for all rates; and the character, permanence, and economy of the work done under the different conditions is a matter of practical importance. Perhaps of chief interest, however, are the inferences suggested by the results as to certain fundamental modes of mental activity, partly apperceptive and partly retentive and reproductive, especially as to the probable subconscious adjustments that occur with the passage of time.

The solution of these psychological questions will aid in the analytic study of the pedagogical aspects of the problem. While the instinctive adaptation of both pupils and teachers, together with existing pedagogical precepts and traditions, has already contributed much to the elucidation of the subject, yet laboratory studies and practical tests in schools will be of service in the analysis of the causal factors, which common experience usually cannot with sufficient certainty accomplish. Moreover, personal differences, the pressure of work, and the common pedagogical illusion that what is easy for the teacher will be so for the pupils are liable to mislead the judgment as to the most advantageous rate of work. One cannot visit schools without receiving the impression that the rate of work of pupils varies considerably in different classrooms, and that the best rate is exceeded in some and not attained in others. Particularly is there danger that the rate will not be sufficiently adjusted for the different degrees of difficulty of the subject matter. The rate of work in many European schools, especially those under government control, seems often very severe and rapid, due, perhaps, in part to extensive programmes of study, and in part to the adoption of a rigorous class management, almost military in character. Whether average differences in the rate of work in different countries is sufficiently great and demonstrably due to permanent, temperamental causes so as to make it significant to speak of national types or differences cannot be regarded as definitely established, though statements with such implications are often made. At any rate, personal differences of this kind within a given country are probably much the greater.

In the analysis and formulation of the problem several distinctions must be noted. Mental states are of many different kinds and succeed each other at rates varying much according to circumstances. The train of ideas which makes up the stuff to be remembered may take its course with or without immediate external suggestion and guidance. In the experiments here reported the succession of mental states was suggested by series of letters, nonsense syllables, associable, and dyssociable words, presented at certain definite rates. The rates of these external series, especially those for the eye, may be made quite accurate; but the corresponding mental series can only be made approximately as definite. The matter may be represented as follows:



Letters,	l	f	k	p	m
Nonsense syllables,	mak	tuj	pif	nox	vol
Unconnected words,	horse	bricks	solder	acorns	string
Associable words,	paper	writing	compose	position	work

Part AB of the imaginary time line AX is designed to represent the external duration of the impression; in the case of vision, for example, its visibility. Part BC represents the vacant interval between the disappearance of one impression and the presentation of its successor. The series of impressions may have been a series of any one of the different kinds of objects represented in the columns below the time line. The curve in the space above the time line is meant to suggest the rise and fading away of the idea in consciousness. In the case of associable ideas we may imagine a back reference from one idea to its predecessor that will connect and intensify them both.

A question of special interest but also of special difficulty is that regarding the nature of the connection established between successive members of the series, the answer to which can only be attempted by certain neurological speculations. Even the general form of the connection has only partly been made out; and it is to the solution of the latter problem that the numerous experimental investigations, beginning with that of Ebbinghaus, have in the last twenty-five years aimed to contribute. Ebbinghaus found that the associative bond extended not only from one syllable to the next succeeding in the series learned, but measurably also as far as the ninth down in the list and also to some distance backwards. He ascertained that the strength of the connection decreased inversely as the logarithm of the time, as measured by the number of readings saved in relearning nonsense syllable series in the course of a month. A similar logarithmic law was found by Wolfe to hold for the decrease in the distinctness of the memory of a tone within the short period of a minute, as measured every few seconds by the ability to discriminate it from another nearly like it, though it must be noted as an exception that the tones were most easily discriminated from one another when the interval between them was about two seconds, which corresponds with the interval Dwelschauvers found most favorable for the adjustment of the attention in reaction time. Müller and Schumann have demonstrated that the associative bond between two syllables of a foot in rhythmic reading is stronger than the bond between adjacent syllables of different feet. Interference effects have been shown to develop in the formation of new modes of reaction for the same sensory material. Müller and Pilzecker have demonstrated with the aid of a new method that, other things being equal, old associations function more slowly than the more recent. The strength of an association varies, besides, with several other factors such as the condition of vigor or fatigue and the rate and mode of learning, which, as far as they concern the experiments here reported, will be considered in connection with their presentation.

The different characteristics of the associative bond are no doubt present in the ordinary operations of memory, though most of them are only to be definitely ascertained by means of experiments. What is on the surface and may be seen within any short period of observation are such facts as are described by the so-called laws of association: contiguity and relation, together with vividness, frequency, recency and the number of repetitions as determining the strength of the association. Inferences from experiments and pathological observations have suggested with more or less probability, besides these, certain less easily discernible factors, many of which will have to be considered in the endeavor to account for the experimental results presented. Thus, Professor Burnham infers from cases of retroactive amnesia that "in normal memory a process of organization is continually going on—a physical process of organization and a psychological process of repetition and association. In order that ideas may become a part of permanent memory, time must elapse for the processes of organization to be completed." If true, this theory would play an important part in the explanation of the results of this investigation. In the cases of retroactive amnesia described by him, the amnesia extends not only to the facts of the accident that brought it about, such as the fall from a horse or other injury, but also to the events preceding the accident by from a few seconds to perhaps several hours. The explanation proposed for this, which is the basis for the more general inference quoted, is that the shock interrupted a certain organizing process of the mind, which is active not only at the time of receiving the impressions but for some time afterwards and is necessary for their retention and recall.

Therefore, while it is possible to make each member of an external series of impressions occupy a definite time and be succeeded by another after a definite interval, and to make conscious attention to the impressions approximately parallel with this, there is the possibility of a more continuous nervous activity, which, in spite of the apparent parallelism of the objective and the subjective series, can make the results with the slower rates better. In view of this possible persistence of nervous activity, it will be necessary not to imply, in formulating the problem, as the study of the effects of changes in the time variables in memorizing, more than that conscious attention to the series approximately corresponds with the rate of the external series.

II.

The methods employed in memory investigations may be placed in three classes, namely, those which involve the complete memorizing and reproduction; those which involve only the partial memorizing and reproduction of the items studied, and those which measure the rate of functioning of the association. In the first class belongs the original memorizing method of Ebbinghaus, in accordance with which a series of nonsense syllables was repeated till a perfect repetition by heart was obtained. The number of readings or repetitions needed was used to measure either the increased difficulty of learning with increased length of series, or, in the case of series once learned, to measure after certain intervals the strength of the associations still remaining by the saving effected in relearning, in comparison with the number of repetitions required for a new series of the same length. Throughout this paper this method will be called the complete memorizing method. In this class belongs also one form of the memory span test, namely, that in which the limits of the span are ascertained by making successive additions to or subtractions from a list of items read or shown to the subject till the maximum number that can be retained after being once heard or seen is found.

In the second form of the memory span test, which is the one usually employed in simultaneous experiments with a large number of individuals, a slightly longer list of items than can be remembered after a single presentation is used and the relative efficiency measured inversely by the errors made. The actual "memory span" is not ascertained but merely the relative efficiency under the same conditions. This experiment is the best known of those which belong to the second group. The reproduction of what is memorized is partial and must be so, since differences in records are only possible when errors are present. The method of right associates (*Treffermethode*), developed by G. E. Müller and Pilzecker in the psychological laboratory at Göttingen and first described in a paper by Jost, in 1897, also belongs in this class. A series of items like nonsense syllables is repeated a certain number of times and, after an interval, alternate ones are shown singly to the subject and he is required to recall the one immediately succeeding or preceding, as the problem may require. Usually the syllable series is learned by reading in trochaic rhythm; and the test of the strength of the association at some subsequent time is made by presenting one of the syllables of each foot to the subject and requiring him to recall the other. The number rightly recalled is taken as the measure. Additional information regarding the value of the associations is sometimes obtained by measuring with the aid of the chronoscope the length of time required for each recall. The prompting method of Ebbinghaus is still another method to be classified in this second list. In this, too, not enough readings are employed to enable the subject to reproduce the material completely; the number of times he must be prompted to enable him to do so is the inverse measure of the condition of the associations. The methods of the second class so far described are, of course, easier and do not require so much time in execution as would the memorizing of series of the same length for complete reproduction, with the possible exception of the method of right associates when the time of the associations is measured. This element of greater ease is still more noticeable in another method belonging to this class, which may be called the recognition method. An account of the employment of it is found in an article by Hegelmaier upon memory for length of lines, an experimental study made in Vierordt's laboratory about 1852. It has since been employed by H. K. Wolfe in experiments upon memory for tones and by many others in the study of memory for qualities and intensities of sensations. The method is the same as the method of "right and wrong cases" in experiments for the Weber law, the interval between presenting the standard and the comparison being varied, if the purpose of the experiment is to ascertain the rate of forgetting. More recently the recognition method has been employed by Fritz Reuther with complex materials. In his experiments he required the subject to read series of four place numbers, which he then showed him later according to certain experimental plans to see how many he would recognize. The subject did not, of course, know that the second series was the same.

The writer has employed the method in the following form: A series of ten nonsense syllables were read once or more according to the problem; then a second series of twenty syllables was shown containing the first ten and ten others. The number of syllables the subject recognized under given conditions was taken as the measure of the strength of the associations. It is essential in the use of this method, as with others of its class, that only a part of the syllables shall be recognized. Figure 2 gives a sample of the material used.

FIGURE 2. PART I.

pub kij mag qer vum sed lif hov tol jap

PART II.

tus ziz pub sed nur vum san wav jap qer
gox lif tol qif hov mag lom ket kij dul

The method employed by Miss Theodate L. Smith, in which a series of ten syllables or characters were exposed to view for twenty seconds for the subject to read as he pleased, with the requirement that he should write down all he could remember afterwards, belongs also in this class; as do the methods employed in the experiments reported in this paper.

In the third class may be placed the usual methods for determining the practice curve by measuring the improvement in speed, as, for example, was done by Bryan and Harter in the study of telegraphic language. Here, too, would be placed the method employed by the writer in the study of memory problems by the interference of associations. In both classes of experiments the rate of functioning is what is directly measured.

III.

The experimental technique in the use of these methods also goes back to the work of Ebbinghaus, who formulated some of the chief requirements. To secure uniform unconnected material for his experiments, Ebbinghaus made use of nonsense syllable series. Müller and Schumann introduced a convenient plan for their construction and laid down certain rules for securing greater homogeneity, so as to make them "normal" or "extra normal," in this developing a procedure which Ebbinghaus had discarded in favor of leaving the character of the syllables wholly to chance. To be "normal" all initial consonants, all middle vowels, and all final consonants respectively in any given series must be different, as far as possible. The initial consonant of the first and the final consonant of the second syllable of one of the trochaic feet into which the series is divided in reading must not be the same; and two or more successive syllables must not form well-known words or phrases. To be "extra normal" no two syllables used on the same day shall have two letters the same. In constructing series small cards with initial consonants are placed in one box, cards with the middle vowels in another, and cards with the end consonants in a third. A syllable is made by taking by chance a letter from each of the boxes in the order mentioned. When a sufficient number of syllables for a series has been secured, the cards used are put back into their respective boxes and the procedure repeated for other series. After this, what needs to be done to make the series "normal," can usually be done by shifting one or two syllables; the "extra-normal," however, may require the construction of new syllables.

To secure the uniformity necessary for experimental purposes in the work of memorizing itself, Ebbinghaus read, repeated what he could, read from the point of hesitation, and continued reading and repeating in this way, all at the uniform rate of 150 syllables per minute, till he was able to repeat the whole series by heart. The rate was secured by timing the reading, in the early experiments, by the strokes of a metronome, and in the later, by the ticks of a watch. The syllable series was written on a sheet of paper, and in reading several syllables might be seen at once. The effort to maintain a certain rate by aid of an external standard is a source of strain and distraction, especially at first; moreover, the possibility of seeing several syllables at once might introduce an error in the study of various values in successive association. Müller and Schumann, therefore, in taking up the study of memory problems by the Ebbinghaus method, endeavored to avoid these difficulties by placing the syllable lists, written in a vertical order, on the horizontal drum of a kymograph, before which they arranged a screen with an opening so that only one syllable could be seen at a time. This plan has been employed not only in the extensive and important studies made in Professor Müller's laboratory at Göttingen, but in studies made elsewhere, as at Würzburg and Chicago. For changes in the duration of visibility of the syllables, or of the intervals between them, or of the rests between series of syllables, it is evidently inconvenient and not free from the introduction of other variables than the one studied. For example, if the rate of presentation of the syllables is varied by changing the rate of the drum, the rate of motion of the syllable, as it goes before the eye, and so the time of its visibility, or else the distance it traverses, is altered. To change the rate of presentation of the syllables and yet keep the time of visibility the same, would require keeping the kymograph at the same speed and respacing the syllables. The authors call attention to another difficulty, namely, that watching the movement of the drum and syllables gives rise to eye strain and even dizziness, which one of their subjects suggested might be remedied by having the presentation of the syllables made step-fashion. Published accounts of apparatus by which this might be done have not, however, appeared till recently. The latest, that by Dr. Wirth, seems to differ from the first, that of Randsburg, chiefly in being so planned as to be nearly noiseless in its operation. It consists essentially of a disc or short drum moved by a weight, but made to move step-fashion by an escapement, which is operated electrically by a metronome. With the disc, the items to be learned are written in sectors, the disc moving forward a sector at a time and each sector being one-sixtieth of the whole area. These pieces of apparatus leave the syllable stationary while it is being observed but do not provide means for varing the duration of visibility independently of the general variation in the rate of succession or for the convenient adjustment of the intervals between series. In these respects the apparatus used in our experiments and described below has the advantage.

Figure 3 reproduces a sketch of the compound interrupter, or system of pendulums, by which the intervals between syllables and the duration of visibility of each syllable is regulated, as far as need be, independently one of the other. The visibility is regulated by the small pendulum at the right, P^3. P^3 does not swing independently of the other two pendulums, P^1 and P^2. At each extremity of its swing there is an electro magnet, one of which shows in the figure. It swings from one to the other only when the current in the magnet that holds it is broken. When the current in the magnet at one end is broken, it is made simultaneously in the magnet at the other extreme, so that the pendulum, when it reaches this, is caught and held till the current in that is in turn broken. The current may be made or broken in these two magnets by either of the two large pendulums according to the connections made. The change occurs when the large pendulum, with which the small pendulum magnets are connected, swings past its centre and therefore regularly at the beginning of each half swing. The half swing of the large pendulums gives the intervals between the syllables, the swing of the little pendulum gives the duration of visibility, which can, of course, remain the same

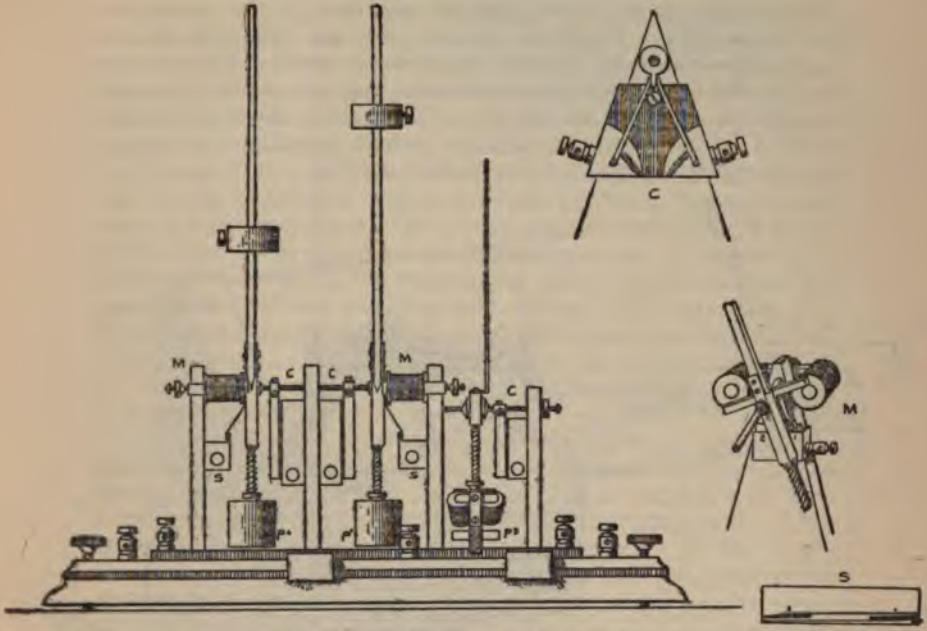


FIGURE 3. COMPOUND INTERRUPTER.

with different periods of the larger pendulums, or be varied while the total interval between the syllables remains the same.

The large pendulums are kept in motion by being parts of the oscillating motors (M). They receive a slight impetus when nearing the vertical in their swing each way. The commutator (S) is so arranged that the contact spring bears now on one side, now on the other, according to the direction of the swing. The heavy lines of (S) are metallic contact surfaces.

For practically all experiments likely to be made, only one of the large pendulums would be needed, so that the interrupter could to that extent be simplified. It would then consist of one large and one small pendulum. The electrical connections with the escapement would be correspondingly reduced in number. In this respect it is now so arranged that by the switch shown in Figure 4 either of the large pendulums can be made to operate the escapement in immediate succession, the small pendulum working with one or the other as may be required. This arrangement was especially made so that the subject, according to a variation of the Ebbinghaus method, might read the syllables at one rate and repeat them at a slower rate, but be able, as soon as he failed in repeating, to throw in the more rapid rate, at any time in the series, for further reading. However, no use of it for this purpose has been made and most forms of experiment would not require it, so that the device for this particular purpose could very well be omitted. As far as known to the writer, this piece of apparatus for combined intervals is new in principle and design and may possibly prove to be of some service.

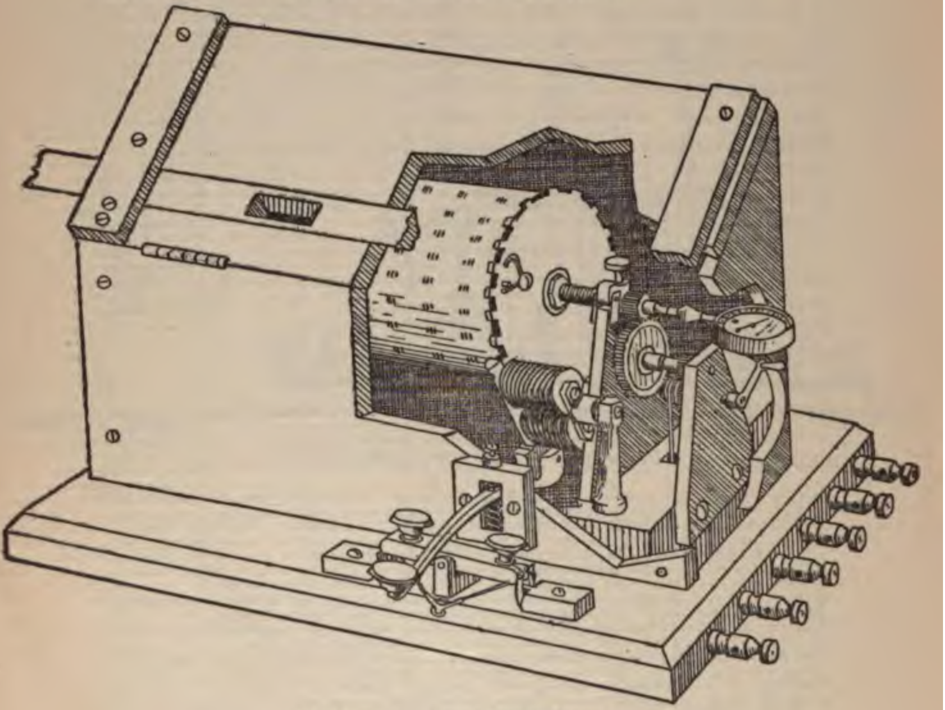


FIGURE 4. EXPOSURE DRUM.

Figure 4 gives a picture of the exposure drum and escapement. The drum is about 16.5 cm. long and 9 cm. in diameter. Since it must act as quickly and with as little shock as possible, a point of special importance is to make it very light. This one has aluminum ends and a thin aluminum cylinder. For the cylinder a heavy paper was at one time used, but the aluminum seems better. The use of a skeleton steel end for the one with the cogs would be an improvement. One thing to be noted is that by the pressure of the lever the escapement is thrown out of action so that the drum flies around to the starting point, being there arrested by the two-pronged spring screwed to the end of the drum, the curved prong arresting the drum gently, the straight one placing it in position. As soon as the end of a series is reached, it is possible to make the interval before beginning reading it again whatever may be desired, without the need of waiting till the drum, by its interrupted motion, brings one to the beginning.

The noise is reduced somewhat by mounting the drum solidly on a rather heavy iron base and by placing the entire apparatus on a stone foundation. But even so, the escapement works with a sharp, though not a reverberating click. However, the blows of the cogs on the escapement are synchronous with the presentation of the syllables and distract the attention but little, since there is no requirement that they shall be attended to or followed. The effect is somewhat the same as would be produced by accompanying the oral reading of the syllables by a synchronous tapping on the table.

One part of the apparatus does not show in the sketch, namely, the part needed in the employment of the recognition method described above. In this case it is necessary to have some means of recording what syllables are recognized. This record is made on a narrow strip on the left end of the drum by means of an electric marker. The use of other parts of the apparatus, such as the slide with opening for seeing the syllables and the rotation recorder at the right end, doubtless, need no explanation. One other thing, however, should be mentioned. No additional apparatus is needed in employing the method of right associates, except a chronoscope in case the time of recall is also to be measured. The pressure on the same key can simultaneously start the chronoscope and make the exposure of the syllable.

The drum was made of such a size that the vertical distances of the syllables, or the interlineage, corresponds to a fixed interlineage of a type-writer, by which all the syllable lists have been prepared.

IV.

As has been stated, the first experimental data were, however, not obtained with the aid of this apparatus but by oral presentation at different rates, the rate being controlled by a metronome. The chief experiment made by Supt. Sanders by this method comprises records obtained simultaneously from thirty students in the psychological laboratory, in May and June, 1895. The students had participated in laboratory practice throughout the academic year up to that time, so that the conditions were quite favorable. Two kinds of materials were used, namely, lists of ten associable words and lists of ten letters arranged in accidental order. Series of both kinds were read on each day of experimentation according to a compensating programme, with intervals from word to word or letter to letter of 0, .5, 1, and 2 seconds. Each series was read once and the subjects were asked to write down as many as they could remember within a certain time beginning immediately after the reading. The average number of series of associable words and letters employed with each individual at each rate was 22.4 and 30.23, or 224 words and 302.3 letters respectively; giving a total of 6,720 words and 9,070 letters for the whole group at each rate, or a total of 20,160 words and 27,210 letters for all at all three rates. The errors made are classified as omissions, insertions, and mistakes of order. By omissions is meant the number of words or letters left out by the subject in reproducing a series, by insertions the number of words or letters put down by him that were not in the original series. The number of real mistakes of order cannot be counted exactly but it is probably approximately proportional to the number of changes that must be made to set the series right, and this is what has been counted and set down under the head of mistakes of order. The accuracy of reproduction is inversely as the number of errors; this must be kept in mind in the interpretation of the averages.

TABLE I.

	Associable Words.			*Letters.*		
Intervals between words.	0.5	1	2	0.5	1	2 seconds.
Omissions.	2686	1967	1286	2439	2362	2160
Insertions,	540	319	186	825	705	624
Mistakes of order.	209	167	134	735	801	703
Total.	3435	2453	1606	3999	3868	3487
Per cent. of whole number of words or letters.	51.12	36.5	23.9	44.09	42.65	38.44

To facilitate comparison of the changes with the different rates, Table II gives the distribution of errors at the different rates in per cents of the total number of errors of each kind.

The number of errors with the associable words is 37.17% of the whole number of words; the number of errors with the letters is 41.73% of the whole number of letters. In the case of the words, the omissions are 29.46%, the insertions 5.16% and the mistakes of order 2.53% of the whole number of words; and in the case of the letters the corresponding per cents are, 25.58, 7.92, and 8.45. The fact that there are relatively

TABLE II.

	Associable Words.			*Letters.*		
Intervals.	0.5	1	2	0.5	1	2 seconds.
Omissions.	45.23	33.12	21.65	35.04	33.93	31.03
Insertions.	51.67	30.53	17.8	38.26	32.73	28.97
Mistakes of order.	40.95	32.75	26.28	32.83	35.77	31.4
Total.	45.84	32.74	21.43	35.23	34.04	30.79

more insertions and especially mistakes of order with the letters than with the words, is doubtless due to the fact that the words make a series with an order determined by the associations, which is not generally true with the letters but only accidentally so when they happen to fall into a mnemonic scheme. That they would not be remembered so well in their order would naturally follow. The absence of definite, well known associations in the case of the letters, together with their small number and their being used over and over again would make one more liable than in the case of the words to make insertions of letters which should not be present.

What concerns us especially, however, is the striking difference in the relative distribution of errors in the two kinds of materials at the different rates. With the words the number of errors decreases to less than half as the intervals increase from 0.5 to 2 seconds. With the letters the decrease is relatively small, the change from 1 to 2 having somewhat greater effect than the change from 0.5 to 1 second. With neither kind of material do the different kinds of errors decrease in the same proportion. In both the decrease in the per cent. of insertions is greatest, in the per cent. of mistakes of order least of all.

The marked reduction of the errors with lengthening intervals in the case of the words can leave no doubt of the great effect of the interval. The reduction occurs with every individual and with regard to all kinds of errors, with a few exceptions in mistakes of order. The number of errors would be still further reduced if the intervals were made longer, for, if the connection between the words is carefully noted, all can usually be reproduced either forward or backward, even after a considerable period of time. Should the reduction in the number of errors continue at the same rate, as is probable, there would be only 10% errors if the longest interval were doubled; and, if the interval were then doubled again, nearly all would perhaps disappear.

In the case of the letters the reduction in the errors with increasing intervals is relatively small and does not occur in every case but only with twenty-one of the thirty subjects, the rest either doing nearly equally well or slightly better with the more rapid rates.

After the tests were over, Supt. Sanders made a personal inquiry regarding the methods of memorizing by the different subjects, particularly as to whether mnemonic aids suggested themselves in learning the letters. He found that most of those who prefer the slower rates have a disposition to employ some grouping or association, while most of those who prefer the more rapid rates do not do so. It seemed probable, therefore, that the better records obtained with the letters at the slower rates were due to associative processes in this case as well as in the case of the words, and that, aside from this the mind can adapt itself to receiving simple unrelated impressions, like the letters nearly equally well at any of the rates employed, while the intervals have a high value with relatively complex and associable material. That this conclusion represents the most important comparative aspect of the two sets of experiments, seems clear. Whether it can be made to cover all the facts now known about this problem, will be considered later in connection with the discussion of subsequent experiments and the results of others.

In the case of the associable words the total number of errors make the following per cents of the whole number of words at the different rates; (0.5, 1, and 2 seconds), namely, 51.12, 36.5, and 23.9, giving 14.62 and 12.6 respectively as the amount of decrease from the shortest to the medium and from the medium to the longest interval. If we consider only the omissions, which are about four times as numerous as all the other errors put together, we find that the decrease from 0.5 to 1 is almost exactly as great as the decrease from 1 to 2, the omissions making the following per cents of the whole, 39.97, 29.27 and 19.14, and giving 10.7 and 10.13 as the differences. The decrease in the errors proceeds approximately in an arithmetical series, while the intervals increase in a geometrical series, the rate of decrease of the insertions being greater and that of mistakes of order less. The number of insertions at the fastest rate is disproportionately large, which may be due to indistinctness of enunciation by the experimenter or inattention on the part of the subject. As has been said, if the relations in the series had been clearly noted, nearly all of the words would have been recalled. The realization of meanings and the noting of relations between the words took place to some extent with the most rapid rate, to a much greater extent with the slowest rate, though it was even with this incomplete. The words represent complex, though familiar ideas and the seeing of the relations between them depends upon and doubtless takes place directly in connection with the apprehension of the different meanings of the words. Moreover, if we assume that the amount recalled varies with the degree of association, we can state the result from this point of view as follows: *The acquisition and retention of a series of familiar associable words varies approximately as the logarithm of the interval at which the words are spoken, the shortest interval being that which barely permits a clear pronunciation.* The length of time before a given stage of association would be reached would vary with the individual and the material, but even within these limits the scope of this law, if we can so call it, may be very great. It is not unlikely that it would roughly represent the rate of acquisition of ideas from reading or oral communication; not that the intervals in this case would be between words, but rather between ideas suggested by significant words, phrases, and clauses. Seeing the relations between the words of these experimental series requires a realization of the meanings of words and the perceiving of relations, which is not unlike the process required for the apperception of discourse. According to this formula, acquisition is especially rapid in the first few moments, which accords with experience, since a speaker needs only to make a small difference in his rate of utterance to make himself either well or ill understood. With respect to this matter, however, individual differences are considerable, and the statement is only regarded as a rough expression of a general result. A preliminary experiment by Supt. Sanders with six persons gives some additional data. Two kinds of materials were used, namely, lists of ten associable words and lists of eight disconnected, or dyssociable words. There are 240 series for each of the six general averages, 40 being supplied by each subject. The intervals do not, in this case, make a geometrical progression, which must be kept in mind in considering how closely the averages follow the formula. The per cents in the the table below (the sums for all three kinds of errors) are of the total number of words for the different intervals.

	Associable Words.			*Disconnected Words.*		
Intervals.	.476	.715	1.43	.476	.715	1.43 sec.
Per cent. of errors.	63.4	51.04	35.45	66.7	56.8	45
Differences.		12.36	15.64		9.9	11.8

To conform to the formula differences with the associable words should be in the ratio 3:5. The actual ratio is 3:4, which shows considerable divergence. There was some, but not so great, a difference in the same direction in the preceding experiment. This may be the normal deviation, though there is some reason for believing that it is due to the development of insertions from causes that would not be a part of the process to which the formula is supposed to apply. Such would be insertions due to misconceptions of words indistinctly spoken, which would be liable to occur chiefly at the most rapid rate. The misconception takes its place as one of the number of items the subject can remember and so will often give rise to an omission as well as to an insertion, in this case, counting double in the totals. Supt. Sanders collected a number of instances of still another type of insertion that occurs chiefly with the rapid rate. In this case the subject, instead of remembering the word spoken, remembers a suggested word.

Word pronounced.	Word written.	Word pronounced.	Word written.
lisp	tongue	music	songs
dual	double	fire	flames
silent	quiet	classes	scholars
trees	forest	sound	noise
son	boy		

With regard to the disconnected words, it might have been supposed that the effect of the intervals with them would correspond rather with the effect on the letters than with the effect on the associable words, but the reverse is true. It will be seen, however, that the influence of the intervals in the case of the disconnected words is only about two-thirds as great, which would place them in this respect between the associable words and the letters. As in the case of the associable series, the words represent familiar, but rather complex, ideas, and besides, they may not be wholly unassociable, but merely dyssociable. The explanation of the effect is therefore probably the same in both cases, though the value of the longer intervals with the disconnected words is probably to be found in the more complete apprehension of the individual words rather than of their connection with each other.

V.

In the experiments made by Mr. Herrington in 1903-4, with the aid of the apparatus described in Section III, only one kind of material was used, namely, nonsense syllables. In this case a greater degree of precision with regard to the intervals is possible; and the schedule of experiments was arranged to make use of this advantage so as to ascertain the effect of changes in all the elementary time variables. It was, therefore, partly a study of the elementary factors of technique and partly a study of the psychological problems involved. The experiments of Sanders were addressed to the ear, these to the eye; and the subjects were tested singly, not en masse as before.

The elementary time variables are the duration of exposure of the syllable, the intervals between syllables (that is, the time from the beginning of one syllable to the beginning of the next), and the rest, or interval from the end of one complete reading of a series to the beginning of the next reading. As the vacant interval from one exposure to the next is the complement of the exposure with respect to the whole interval, it will not need separate treatment.

Mr. Herrington's work comprises thus three sets of experiments, one for each of the time variables, with records from three subjects in each case. Twelve syllable series were used throughout. Each was read four times under conditions required by the problem, immediately after which, within a specified time, the subject was required to write down as many syllables as he could. Forty-eight series were studied, that is, read silently by each person at each rate. Each day he was given twelve series to learn. These were distributed equally with respect to the different rates and taken in such an order as to compensate as far as possible for practice and fatigue, so as to permit at the end a safe comparison of the records under the different conditions.

The first set of records concerns the value of variations in the time of exposure, or visibility, of the syllable, when the total interval from syllable to syllable remains constant. The exposure was reduced till it seemed barely possible, without practice, to read a series correctly. This exposure was 41σ. The exposure was then increased by doubling so that the duration makes a geometrical series, all but the last, which was erroneously made a little too small for this purpose. The total constant interval from syllable to syllable was 768σ. By subtracting from this the different exposure times, the length of the vacant intervals from the end of one exposure to the next may be obtained. The following table gives the average number of errors of all kinds, per series, together with the probable

TABLE III.

Exposure.	41σ	82σ	164σ	318σ
J. L. M.	13.6 ± .25	12.8 ± .34	13.7 ± .03	13.1 ± .31
G. A. H.	10.7 ± .23	10.1 ± .03	10.7 ± .32	10.8 ± .35
J. H. W.	10.7 ± .23	10.2 ± .23	10.5 ± .19	10.7 ± .22
Average.	11.66	11.03	11.63	11.53

errors of the averages, the latter being calculated from the records grouped into six averages of eight each to compensate for practice effects.

It will be seen that the variations in the exposure appear to affect the results but little. However, a well marked preference was expressed by the subjects for the exposure of 82σ, though the actual results do not indicate any important difference in its favor. It was said to be more agreeable and restful than the others. It is on account of this preference that this exposure is used in the subsequent experiments with other variables. The 318σ exposure was felt to be unnecessarily and somewhat disagreeably long and the 41σ as so inconveniently short as to involve a strain.

The apprehension of a syllable is so nearly purely a preformed apperceptive process that the longer duration of the visual image seems to affect the result but little. In fact the process appears to have a period of its own so that the duration of the objective stimulus beyond a certain point is felt to be disagreeable. The process of apprehension does not end with the exposure; for, as will be seen in the next experiment, the length of the vacant interval before the next exposure affects the results very greatly, but for this the objective stimulus is not needed and is likely to be felt as a source of distraction.

There is still another way, besides through the duration of the visual impression, that we might expect that varying exposure would affect the results, that is by inducing a more or less tense attention according as the syllable must be recognized in a very short or a relatively long time. That this would affect the records favorably or unfavorably, is not impossible. Another set of records would, perhaps, give more marked differences than were obtained this time, especially since the subjects learned so few of the syllables that a difference of a single one represents a large fraction of all they learned and the test could be made more sensitive either by reducing the number of syllables or increasing the number of repetitions. The comment of one subject on the 318σ exposure suggests another possibility, which it would require additional experiments to test. He said this exposure was unsatisfactory because it was too long for one thought of the syllable and not long enough for two. Should the hypothesis implied prove correct, there might be several exposures of a certain periodicity that would appear agreeable, though it does not seem probable that a second or third look would enable the subject to make better records than he would make with the same number of purely mental repetitions.

In the second set of experiments the exposure is kept constant at 82σ, but the interval from the beginning of one syllable to the beginning of the next is varied. The vacant interval from the end of one exposure to the beginning of the next is, of course, obtained by subtracting 82σ from the total interval in each case. The method and programme of work is similar to that of the preceding experiment. There are 48 records for each average, or a total of 432. The subjects were requested not to repeat the syllables more than once, which they did silently at the time of exposure, and to make an effort to keep the mental processes parallel with the external series given by the apparatus, so that the results obtained might not be due to conscious utilization of the longer intervals for extra repetitions. It was with the variable here considered that the experiments of Supt. Sanders dealt, though with different materials and methods. Below is a table of the average number of errors per series.

TABLE IV.

Total intervals.	384σ	768σ	1536σ
W. W. C.	11.6 ± .27	10.4 ± .19	10.1 ± .34
J. R. P.	9.3 ± .14	8.7 ± .14	6.5 ± .13
U. M.	10. ± .25	7.6 ± .19	5.9 ± .13
Average.	10.3	8.9	7.5

The effect of increasing the intervals is greatly to decrease the number of errors. The short interval is nearly the minimum for persons without special training, but the indications are that an interval longer than any employed would have still further reduced the errors.

In the third set of experiments the rest, or interval, between the end of one reading and the beginning of the next reading of the series as a whole, is the object of investigation. A procedure corresponding with that of the preceding experiments is employed. As before, the table gives the average number of errors per series, of which there were 48 for each average, or a total of 432. The exposure was 82σ, and the interval from the beginning of one syllable to the beginning of the next was 556σ. The subjects were asked to avoid recalling the syllables in the rest period; and two of them employed devices like whistling or humming tunes to enable them better to conform to the requirement.

It is clear that the duration of the rest between readings can, also, greatly affect the results. The length either of the intervals between syllables or of the rests between readings need therefore to be regulated carefully in memory experimentation.

Both longer intervals between syllables and longer rests be-

TABLE V.

Rests.	0	30	60 seconds.
M. A. C.	9.3 ± .34	8.7 ± .19	7.4 ± .23
W. T. S.	9.5 ± .27	7.6 ± .02	6.7 ± .03
M. S.	9.4 ± .03	6.9 ± .37	5.8 ± .17
Average.	9.4	7.73	6.63

tween readings increase the average time between the reception of the impression and the effort to reproduce it. Other things being equal, we should expect this to cause an increase and not a decrease in the number of errors with the longer intervals and rests, since we are disposed to assume that a nervous impression begins to fade away soon after it is received, in case it is left to itself and not further elaborated and connected by attention, which accords with many facts both of the laboratory and of common experience. If this assumption is true, then the advantage of the longer intervals and rests would consist not of greater strength from the independent development of the impression, but of greater strength either from clearer first impressions, or from more adequate association, or from the development of tendencies to recall, or perhaps from still other factors, less known, but like these, favored by a longer time. While it would still be necessary to recognize these factors if this assumption were not true, since they have undoubted value; yet, if the assumption is not true, an hypothesis with regard to the effect of the passage of time upon an impression could be framed that might be a partial explanation of the results. If, for example, the contrary hypothesis were plausible, namely, that an association, like an organ of the body in its development, grew stronger the older it became, then a longer average time between impression and reproduction would be of advantage.

Whether this hypothesis is involved in Jost's law is uncertain, since he emphasizes especially the matter of repetition, and would explain the value of rests between readings by the theory that the repetition of older has a greater value relatively than the repetition of younger associations. Such a specific value might, of course, be due to continued growth of strength of the association with time or to a greater efficacy of the act of repetition itself in the case of older associations. The latter alternative is more strongly implied by Jost's phraseology. If such a peculiar process should actually exist, it is likely that it would be due to a special development in mental evolution. Its general effect would be partly to counteract the disadvantage of forgetting in the case of the associations which experience required us to renew. This might possibly be of sufficient practical importance to make the process subject to natural selection. However, several other factors, whose efficiency is undoubted, need to be taken into account; and such an hypothesis should not be resorted to till these are exhausted.

Of all the errors made by the subjects in the tests for the effect of different exposures, 70% were omissions, 26% were insertions, and 4% were mistakes of order. These per cents vary considerably with the different persons but the distribution of errors for each one with the different exposures is about the same.

With varying intervals between syllables, the per cents of the different kinds of errors are 79.66, 17.33, and 3; and we note here, too, wide individual differences. But there is, in addition, an important difference in the distribution of errors with the different rates, as will be seen in the following table.

TABLE VI.

Intervals.	Omissions.	Insertions.	Mistakes of order.
384σ	39.8	36.1	21.1
768σ	33.5	32.2	30.3
1536σ	26.6	31.7	48.6

The distribution of the mistakes of order, which, however, are only 3% of the whole and so do not greatly affect the general average, varies inversely as that of the omissions. This is true of all three subjects. The distribution of the insertions is not so definite.

It appears, then, that the longer intervals serve especially to enable the subject to fix in mind a larger number of syllables but that the place and connection of the syllables grows less distinct with the longer intervals. We should not have such a result if Jost's law were true of all kinds of associations, including those determining the order; and another hypothesis would fit the facts of the above experiment better, namely, that the longer intervals, through keener attention and longer time for association, permit a better associative apprehension of the syllables. Even if definite mnemonic words are not suggested, bits of words, familiar letter sequences, or other forms of more or less conscious incorporation with existing neural paths may be brought into action if there is time enough. If this hypothesis is true, the explanation of the value of the intervals with the nonsense syllables would be similar to that given for associable and particularly dyssociable words.

In the experiments with rests between readings the distribution of errors is, omissions 79.3%, insertions 17%, and mistakes of order 3.7%. The two subjects (W. T. S. and M. S.) who show the most marked effects from lengthening rests show also an inverse relation between omissions and insertions, corresponding in degree with that noted above. The third subject (M. A. C.) had only one per cent. of mistakes of order. His method of memorizing seemed to be to form a visual image of the whole series and he appears to have forgotten both the syllables and their order at about the same rate.

VI.

The relation of these experiments to those of others has already been considered in Sections II and III, as far as apparatus and technique are concerned; but there remains the task of discussing briefly also the place of the problems and results. The possibility of such investigations is suggested in the orderly procedure employed by Ebbinghaus; moreover, he made the following observations, which have been the starting point for several other investigations in this field. In a series of experiments he learned 12 syllable series on one day and then relearned the same series on the three succeeding days. The average number of readings required for an errorless repetition was 17.5 the first day; 12, the second; 8.5, the third; and 5, the fourth. In another experiment he continued reading and repeating 12 syllable series longer than was necessary for the first errorless repetition, in fact till he reached a number about four times as great, that is, a total of 68. On relearning the series after 24 hours, he found that he, nevertheless, needed 7 readings. On comparing the two facts it is found that the final relearning in the first case was preceded by 38 readings and repetitions, distributed over three days; in the second, by 68 taken all on the day before. This appears to be the first observation on the value of rests between readings of series of syllables.

On the value of intervals between syllables, the first observation published seems to be that by Miss T. L. Smith in her study of "Muscular Memory," though perhaps intervals between readings as well as syllables are included. In these experiments ten syllable series were exposed to view for twenty seconds for the subject to study, immediately after which he was given 70 seconds to record what he could recollect. The subjects were at liberty to read at whatever rate they pleased and they selected quite different rates, one reading the series through once or twice, another four or five times, the rest being between these extremes. The subjects that read most slowly were found to have made the best records. This might be due to personal differences in retentiveness, though there is considerable probability that the difference in rate contributed largely to the result.

With regard to the effect of variations in the exposure of syllables, the apparatus so far employed by others has not permitted accurate experimentation, since changes in exposure have also involved some other variables as well.

On other aspects of the problem considerable material has accumulated. Attempts have especially been made to study the related question of economy in memorizing. Thus, Ebbinghaus reports experiments in which he memorized stanzas of Schiller's translation of the Æneid at the rate of 200, 150, 120, and 100 iambics per minute, and found he learned the same number of lines in 138, 148, 160 and 180 seconds, respectively, which shows that the amount of time required was inversely as the speed. On relearning these lines, presumably at a single rate, twenty-four hours later, he found that 90, 89, 96, and 99 seconds were required, which shows with regard to the permanence a slight difference in favor of the more rapid rates. The question of economy would, of course, also require the consideration of the degree of exhaustion produced by the different rates; and from this point of view the more rapid rates might not be preferable. It will be observed that a greater number of repetitions is required with the rapid than with the slow rates to accomplish the same result. Wherefore we may infer a general correspondence with the results reported in this paper, since repetitions at a slower rate have greater value. However, there may be a still more intimate correspondence with the results for the acquisition of easily associable material discussed in Section IV. The intervals from the beginning of one to the beginning of the next iambic were .3, .4, .5, and .6 seconds respectively for the different rates. The number of repetitions of the lines at the respective rates is therefore proportionate to 46, 37, 32, and 30. If we assume that, when the lines are learned, that they are learned to the same extent with each rate, then the associative value that must be assigned to each interval is nearly proportional to the logarithm of the interval, the products of the number of repetitions and the logarithms of the intervals being proportional to 219, 224, 224, and 233. This fact suggests a correspondence with the law stated in Section IV, that the rate of acquisition varies as the logarithm of the intervals between the items of easily associable material, and thus extends its application to other material and at the same time makes it somewhat probable that it is true not simply for one single repetition, as in the experiments of Table IV, but also of a considerable number of repetitions, as in these experiments of Ebbinghaus.

The experiments of Jost deal with the value of the distributions of readings and repetitions in memorizing, the problem proposed by the early observations of Ebbinghaus, which Jost's results in part serve to confirm and in part to extend. He makes use of two methods, at first the complete memorizing, then the right associates method, and finally the two in conjunction, thus obtaining records whose difference makes the point of chief interest in the paper. By the complete memorizing method he finds with two subjects that ten readings of nonsense syllable series on each of three successive days makes the memorizing of the series on the fourth day easier than do thirty readings on the day immediately preceding, though the difference is small. By the right associates method he finds with two subjects, when twenty-four repetitions are distributed equally on 3, 6 and 12 days respectively, that the most extended distribution, that of two repetitions a day, gives the best results. He formulates and would explain his own results, and the corresponding results of Ebbinghaus as well, by the law that the repetition of an old has relatively a greater effect than the repetition of a young association. The possibility of applying Jost's formula in the case of our results has already been considered in section V.

With the complete memorizing method in the study of these and many other problems a repetition of old associations is involved in the final test, which is not the case with the method of right associates, for here the subject is merely called upon to reproduce associates. If both methods are employed together to test memory for series learned a longer or shorter time ago, the complete memorizing should, according to the law, give relatively better records with the older series, an hypothesis which experimental results from two subjects verify. The difference in the results by the two methods could be explained by Jost's law, but also by the hypothesis that it was due, not to the greater value of repetitions of old associations, but rather to a more rapid decrease of the voluntary revivability of the syllables, which is the thing tested by the method of right associates, than of retentiveness. It is a matter of common experience that the ability to revive, or recall, disappears quickly and long before other evidences of some degree of retention, such as the ability to recognize the object if it is brought up in some other way; and Müller and Pilzecker have shown that, when the same number of right associates are recalled in the case of older and more recently learned series, a longer time is required with the older, indicating greater difficulty. One other thing should be noted. Jost deals only with rests between readings of series; but the effect of longer intervals between syllables seems to be the same as that of longer rests. If the effect is due to the greater elaboration of the same process in both cases, which is not certain, but seems probable, then we have in experiments of Section IV such favorable results as Jost believed to be due to the repetition of old associations in a case which involves no repetition of old associations, but merely a longer time for assimilation.

The part of the work of Lottie Steffens which especially concerns our problems consists of some experiments on the effect of different distributions of six readings within the same period of time. The series were read from a drum, which revolved 12 times in 78 seconds. The most expanded distribution of the series was obtained by reading them every other revolution of the drum; the next grade, by having three readings, then an interval of three revolutions, then three readings and again an interval of three revolutions; the last grade, by having all six readings at first, then an interval of six revolutions. It will be observed that the readings, except in the first case, are more or less massed, which leads Miss Steffens to introduce the following explanation of the better results she obtains with the more widely distributed order. In proportion as the readings are massed, the strength of the associations for the time being is greater; but the stronger the associations the greater is the absolute loss of strength by the time the series in these experiments were to be completely memorized. Since, according to Ebbinghaus, the loss is proportional to the number of readings, the resultant of massing six readings at the beginning is to make the foundation for the complete memorizing of the series not so strong as in the other cases. This explanation would apply with such an arrangement of readings as is employed by Miss Steffens, that is, with the massed readings placed at the beginning of the period; but it would not apply when the massed readings are placed at the end, as in the case of the experiments of Ebbinghaus and Jost, in which, nevertheless, the expanded distribution proved more advantageous. If, on the other hand, the loss of strength were relatively greater to a sufficient extent with the massed than the distributed readings, this type of explanation might be generally valid; but of this there is no certain evidence, since in experiments which might give information on this point there is always the very probable alternative, as an explanation of the differences observed in favor of the distributed readings, that the later numbers of massed readings do not have so great a value as the earlier.

Ogden deals directly with the problems under consideration. His work comprises six parts, the first four dealing with memorizing nonsense syllables, the last two with memorizing poetry. The apparatus employed was similar to that of Müller and Schumann, the syllables being presented back of an aperture by a kymograph drum. The different rates were obtained by changing the rate of rotation of the drum and in experiments with constant intervals between readings, partly by spacing the syllables. Observations on varying exposure and rests between readings were inconclusive, as other variables entered in. The chief groups of experiments deal with the effect of varying the interval between syllables. In the first group, the time from syllable to syllable varied from 2.6 to .9 seconds; in the second, from 1.026 to .325, each in five successive steps. The exposure varied simultaneously in both sets from .705 to .289 seconds, the rests between readings remaining constant at 2.8. Of these two variables, that which affects the result the most is probably the rate of succession. To ascertain the effect of the different rates upon the permanence of the impressions as well as upon learning in the first instance, the series were relearned, usually after fourteen days; a difference in this respect was, however, not clearly demonstrable. The most general result is that with the quicker rates more readings are required but not so many but that the series may be learned in less time than with the slower. Thus subject K required from 12 to 20 and from 7 to 15 readings, respectively, in two experiments in which the intervals varied from 2.6 to .9; and from 13 to 33, respectively, when the rate varied from 1.026 to .325. However, the total time required shortened from 394 to 263 and from 222 to 200 and from 214 to 213.

Ephrussi's investigations fall into two divisions, the first dealing with learning by whole or part, the second with the effect of variation in the rate of memorizing. Ebbinghaus' experiments upon the effect of varying the rate of learning verses of Schiller's translation of the Æneid is repeated with three subjects and with results that agree roughly with his. However, the most interesting fact brought out by Ephrussi's work is what he calls the paradoxical result, namely, that the right associates method gives the best records with the slowest rate, while the complete memorizing method does the reverse, in tests with nonsense syllables. Series of syllables were read at different rates for a given period, at the end of which some were memorized and some were tested by the method of right associates. It is in this experiment that the paradoxical result appears, for the series are memorized more easily but give less right associates after the rapid readings. The chief reason assigned is one he verifies experimentally, namely, that the decrease of the number of right associates takes place more quickly after the rapid than the slow readings. The complete memorizing is done by continuing the readings of the preparatory period, while an interval of five minutes occurs before the test for the right associates, which would permit the more rapid reduction succeeding the quicker rate to produce the paradoxical result. The difference in results with these two methods by both Ephrussi and Jost gives a glimpse of a greater complexity and independence of processes than might at first be supposed to exist.

Ephrussi employed the kymograph apparatus of Müller and Schumann, but in the more recent experiments of Reuther at Leipzig a new type of apparatus, that developed by Randsburg and Wirth is brought into use. As was stated in the discussion of apparatus, the syllable is presented in this not by a continuous but by a step-wise motion of the disc or drum, resembling that used in our experiments, except that it does not have means for the independent variation of the exposure and the rests between readings. Thus, in his experiments upon "the amount retained as a function of the duration of exposure," he does not refer to a study of the effect of varying the exposure of a syllable while the interval between syllables remains constant, as in our experiments. This experiment of his corresponds rather with our experiment upon the effect of different intervals between syllables. For his items are written on every other sector of the Wirth apparatus, which moves forward uniformly a sector at a time. By exposure he means the time a sector bearing a number is present back of the aperture. This is followed by an empty sector, which remains the same length of time, to be in turn succeeded by another number, his "*Expositionsdauer*" being thus equivalent to one-half of the interval between the numbers. He thus has two variables, of which the total interval between the numbers is doubtless the more important. His experiment does not wholly correspond with ours, since in ours the exposure is kept constant. The total intervals employed by Reuther were from one-half to three seconds in length, with five intermediates between the extremes. Two of his three subjects show that the lengthening of the interval is advantageous; with the third, the effect is not so definite as he has at first better then poorer records, with the optimum at one second.

The memory material and the methods must be kept in mind in comparing these results with others. The items were four place numbers, which were read one or more times and then shown to the subject again with the requirement that he should indicate which ones he recognized as having seen before. The intervals between reading and opportunity for recognition was in this case five minutes.

With similar materials and methods, Reuther also made experiments with two subjects upon the rest period between readings, which corresponds with the last of our experiments.

The rests he employed were 4 seconds and 1, 2, and 5 minutes. There is a slight indication that there is a critical period up to which the rests are increasingly beneficial and beyond which this advantage diminishes. Such a critical point has not been observed in other experiments, though rests have been made very long, as in the case of Jost's experiment in which 24 readings were distributed over 12 days and the widest distribution, that of two a day, found to be the best.

VII.

Even with the simplification introduced by experimental conditions, the problem is evidently complex; and it is desirable, as far as we can do so, to attempt to ascertain the relative scope of the different factors involved. As to what they are, as the historical survey shows, several theories have been proposed, usually to account for certain experimental results, and so often not adequate for all. If true, they cannot all be complete but will rather be supplementary. The different factors suggested by these theories and by other possible explanations may be classified into four groups; first, conditions aiding or obstructing the reception and association of the impression; which we will call the apperceptive factor; second, changes and relative variations in retention; third, modifications in the recalling of impressions; fourth, general conditions, such as fatigue or a general organizing activity.

As has been stated with some detail in Sections IV and V, it is the first of these factors, the apperceptive, that seems especially concerned in the production of the results here represented. The material which consisted of simple letters, nonsense syllables, dissociable and associable words, makes a series of increasing apperceptive complexity. The relative ease with which these can be memorized and the effect of the varying intervals seems to be proportional to the amount of apperceptive apprehension of the material and its connections. Even in the case of the letters and nonsense syllables, some evidence was given that the advantage of longer intervals was due to associative processes. While general vigor and fatigue may make the neural disposition more or less strong, they certainly in large part bring about the results they do have by extending or restricting apperceptive processes. The persistence of an impression, or its tendency to maintain itself in consciousness for a time, is also a more or less automatic activity that must be taken into account, as must also the supplementary voluntary attention to the impression. One effect of both of these processes is to vary the degree of apperception. Whether there is any further effect, as far as memory is concerned, can probably neither be asserted nor denied, at present.

That marked changes in the retention of an impression occur as the interval between its reception and reproduction increases, is well known, the most general feature being represented by Ebbinghaus law of inverse logarithmic decrease of strength. As has already been stated, Jost, Steffens, and Ephrussi would explain some results in this field by making certain hypotheses as to changes in retention. Some objections to the application of Jost's law to our results have already been stated; but it may very well be required that the explanation preferred should be tested by his results upon the effect of different distributions of readings, as well as by our own. The same may be said of the theory of Steffens. The underlying premise of both these explanations, that readings or repetitions, whatever their number, up to a certain limit, which in their experiments need not exceed the number necessary for the first perfect repetition, have an equal, or nearly equal, value, is, as far as they have been tested, the least certain of the original results of Ebbinghaus. Each reading may, in fact, have a decidedly greater value, if it is well spaced off from others, than if it is one of a considerable number made in immediate succession, primarily because a higher degree of assimilation could take place each time. The interpretation of the results presented in this paper would imply that this is the fact.

On the other hand, that changes in retention or recall are the cause of the differences in the results obtained by Jost and Ephrussi in the simultaneous use of the complete memorizing and right associates methods described in Section IV seems probable. At least, these differences develop with the lengthening of the time between the impression and its reproduction; and in the case of the results of Jost, if his hypothesis of the relatively greater value of the repetition of old than young associations be regarded as improbable, the effect should perhaps rather be ascribed, as was suggested, to a modification of reproduction than retention. With regard to the observation of Ephrussi that the right associates decreased more rapidly with time if the series were read at a rapid rate, a plausible explanation would connect this also with changes in retention or recall, namely, that a qualitative difference was produced by slow and fast rates, so that in the first case there was more of an incorporation with old and permanent associations while in the second it was more a matter of immediate sense memory, which would disappear much more rapidly. While the results observed would be primarily due to differences in the degree of apperception, the immediate cause would be the relative difference in the retention, or rate of forgetting. However, what the effects of different rates upon retention may be, is not wholly clear, for the few results of other investigators are not conclusive regarding this point.

The influence of possible modifications in the process of recall have apparently not been considered in the various theories regarding the effect of rate in memorizing. As has been suggested, such a modification may account for certain results.

In still another way the reproducing processes may play a part. Miss Smith has shown that repressing or executing the verbal or manual expression of the ideas to be memorized will produce a difference of 15 to 20% in favor of the expression. This expression, which may be regarded as the first recall or reproduction, comes last in reading; and may, with rapid rates, be partly repressed, leaving the reproducing tendencies undeveloped and having an effect comparable with that found by Miss Smith with voluntary repression. Definite observations as to the influence of this factor have not been made, and it is proposed merely as one possibly present in varying degrees with the rapid rates, and serving not merely to create a disposition to recall, but also to keep the idea present for more complete apperception.

With regard to the theory of the existence of a more or less unconscious organizing process continuing some time after the impression has been received and necessary for its permanence and revivability, our results afford some evidence. Our subjects were requested to read the syllables when presented, but to avoid voluntarily recalling them, however much time might be at their disposal for so doing. In experiments of Table V two subjects employed devices to distract the attention during the periods of rest. The fact that the influence of longer intervals and rests is, nevertheless, very marked, makes it somewhat probable that the processes involved, except in their inception, are automatic. With regard to their continuance and dependence upon attention, we must note that the rapid displacement of one impression by the next interferes greatly with memorizing; the organization would, therefore, seem to take place chiefly, perhaps wholly, while nothing else in the shape of a definite impression was before the mind, though there can be general distraction. The variation of the results throughout with the degree of possible apperception suggests that it is this mode of organization that is taking place. The mode of organization might, of course, be of a wholly different character, perhaps not associative at all, but rather like the setting of cement or the adaptation of a plant to a certain curvature, which would also vary with the time allotted. But, however much such figures may be used in regard to the operations of memory, no definite evidence has yet been presented that such processes take place.

That fatigue, in its triple character of auto-intoxication and reduced cell energy and nervous tension, may greatly affect memorizing ability is certain ; and it has probably more generally than anything else been supposed to be the explanation of the difference in value of the massed and widely distributed readings. It is necessary to distinguish from fatigue obstructions resulting from the limits of the memory span, from the persistence of impressions or the interference of associations, or from adaptation, or from other normal processes of the mind. When this is done, the explanatory value of fatigue will be found to be quite limited. In these experiments the effects of general fatigue were counteracted, as far as the averages are concerned, by the programme of work ; and that it should seriously enter into the results of a single record, does not seem probable, especially since other clearly efficient factors are in evidence.

In so far as these experiments bring us into a closer view of the general, practical problems involved, they serve especially to emphasize the predominant importance of apperception in memorizing and the value of sufficient time for its development.

The number and intensity, and so permanence and revivability, of the associations formed may be regarded as a quantity; and a solution of the problem of time economy in memorizing may be attempted with the aid of the formula for the rate of acquisition, proposed in Section IV, and from other data. The general statement of the problem will be, through what means can the greatest amount of apperception of material take place in a given time; though only the effect of a greater or less number of repetitions will be considered. The amount of apperception is a function of the interval between items and also of the number of repetitions; and if it varied directly and equally with each, there would be compensation so that the result of any practicable number of repetitions in a given time would be the same as that of any other, since the intervals and repetitions vary inversely. As a matter of fact, apperception appears to vary as the logarithm of the interval; and, if we assume that it continues to vary at the same time in simple ratio with the repetitions, it will be greater in amount the more numerous the repetitions, within certain limits. If n, $\frac{n}{2}$, $\frac{n}{3}$, represent the number of repetitions in a given time, t, $2t$, $3t$ will be the corresponding intervals, and the amounts of apperception will be proportionate to n (log. t), $\frac{n}{2}$ (log. $2t$), and $\frac{n}{3}$ (log. $3t$) etc., quantities which decrease rapidly at first then more slowly.

It is conceivable that the value of successful repetitions would so change as to compensate more or less for the relatively greater effect of the shorter intervals, but there is no evidence that this occurs completely, but rather evidence to the contrary, as has been indicated in the review of the investigations dealing with this problem; so that on the whole it is probably possible to memorize more quickly with the more rapid repetitions.

Nevertheless, there is great difference in the relative value of repetitions and intervals with different kinds of materials and also with different types of memory, so that a high degree of uniformity in this respect cannot be expected.

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THE INITIAL TENDENCY IN IDEAL REVIVAL.

By Felix Arnold, Ph. D.

The question concerning the direction in which the tendency towards reproduction in memory realizes itself, has received rather scant attention, the general opinion being that memory runs in a forward and not in a backward direction. This idea of serial revival is, moreover, closely connected with an atomistic conception of consciousness, which conception has greatly influenced discussion and experiment in this connection. Now the atomistic conception of consciousness, that, because ideas are revived in a series, therefore, they exist serially in consciousness, has greatly influenced the various points of view when the question of the direction of revival in memory has been in question. I have collected a number of data with the sole purpose of determining this order of revival, and have attempted to infer certain conditions which, it seems to me, must exist, if the kind of revival occurring is to be explained. Before, however, presenting my small contribution in this connection, I have thought it advisable to present the atomistic views so ably given by Hartley and Herbart (two mountain peaks among early psychologists), and also the more modern work done by Ebbinghaus, Müller and Schumann and Müller and Pilzecker, on the problem of the '*initiale Reproductionstendenz.*'

I.

Though Hartley may be considered as the founder of English associationist psychology, and somewhat 'out of date,' still in certain parts of his remarkable work, he offers us the closest kind of psychological analysis. His cerebral explanations are somewhat old-fashioned and impossible from a purely physiological point of view, and one who reads his *Observations* may tend to the belief that the psychological analyses are equally infirm and old. As far as I am concerned with Hartley at present, I shall only give his reasons for the existence of a forward and not a backward revival in memory.

As every one knows, Hartley tries to explain all manner of mental connection, development and growth, by association. In any serial revival this same principle is called in to show why the direction is always forward, the explanation being given in cerebral terms. Any series of impressions

$$A,\ B,\ C,\ \ldots\ldots \text{etc.}$$

will give rise to the succession of ideas.

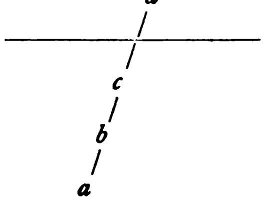
$$a,\ b,\ c,\ \ldots\ldots \text{etc.}$$

and in the same order. For, "the successive impressions of A and B sufficiently repeated, will so alter the medullary substance, as that when A is impressed alone, its latter part shall not be such as the sole impression of A requires, but lean towards B, and end in b at last. But B will not excite a in a retrograde manner, since, by supposition, the latter part of B was not modified and altered by A, but by some other vibration, such as C or D. And as B, by being followed by C, may at last raise c, so b, when raised by A, in the method here proposed, may be also sufficient to raise c. . . . It seems also, that the influence of A may, in some degree, reach through B to C; so that A of itself may have some effect to raise c, as well as by means of b. However, it is evident that this chain must break off, at last, in long successions, and that sooner or later, according to the number and vigor of the repeated impressions."[1] This is a cerebral explanation of forward succession, and has in it the implication of the doctrine of relativity. It also presents for the first time the question of 'mediate' association.

A similar conclusion concerning the direction of mental revival is reached by Herbart. In any series of ideas,

$$a,\ b,\ c,\ d,\ \ldots\ldots \text{etc.}$$

as a is followed by b it is closer to b than to c which forces b below the threshold of consciousness, as b forces a below. And so on with the rest of the series. Graphically represented, when d is in consciousness, the other ideas are in the following position, their position from one another indicating the closeness of the connection:



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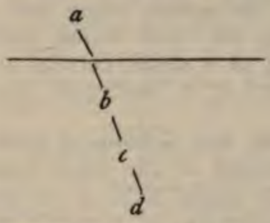
[1] *Observations on Man*, I, 69.

When, by removal of the pressure from other ideas, or because of reinforcement, a has once more a chance to emerge, it does so in the manner represented below, dragging with it in order, those most closely connected with it :



"But the series never runs backward."[1] The order of succession followed in presentation is preserved in revival.

It is evident that with this atomistic conception of consciousness, no other view of revival is possible. Given a series of ideas, a, b, c, d, etc. b follows a, c follows b, and so on. Any initial tendency is impossible. How on earth could any connection from d back to a be possible without a hop, skip and jump which even Herbart's *Mechanik* could hardly stand. In such a series, *d must* be followed by e if there is to be order and harmony in our mental universe. At least, so say these psychologists. I do not, at present, wish to stand for any special kind of revival. But it seems to me that this serial revival has led to a wrong conception of the present moment of consciousness. The revival of our ideas in serial order does not necessitate our consciousness being a filled cabinet, out of which the ideas drop, one by one, any more than the spinning of the threads by a spider presupposes two spools of silk rolled up in the spinnerets. I shall return to this point after I have presented my data in Part II.

Ebbinghaus pursues a different method in his investigation of memory, in the course of which he touches upon the direction of mental succession in ideal revival. While Ebbinghaus does not establish the existence of an actual succession in a backward direction, still he tries to show that there is a connection between two syllables learned which facilitates the process of remembering when the same series is learned in the reverse order. Thus, after having learned a series in the order,

$$I_1,\ I_2,\ I_3,\ \ldots\ldots\ldots\ I_{15},\ I_{16}.$$

more time was saved when learning the series in the order,

$$I_{16},\ I_{15},\ I_{14},\ \ldots\ldots\ldots\ I_2,\ \ I_1.$$

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[1] *Text-book*, p. 23 (tr. by M. K. Smith).

than when learned in a mixed order, thus :

$$I_{16},\ I_{14},\ I_{12},\ \ldots\ldots I_4,\ I_2,\ I_{15},\ I_{13},\ \ldots\ I_3,\ I_1.$$

Since, in the second series 12.4% was saved in the time required for learning the series, while only 5% was saved in the time for learning the third series, Ebbinghaus concludes "that in the learning of a series, certain connections between the members is formed in a backward as well as in a forward direction."[1]

Against this, Müller and Schumann give with approval the criticism of Münsterberg to the effect that when we learn a series of syllables, the visual field is not restricted to a single syllable, and unconsciously, in glancing at the syllables, we form such backward associations as will account for the difference in the saving of time given by Ebbinghaus. Müller and Schumann avoided this difficulty by presenting the syllables in isolation, thus minimizing the visual concomitance. They come, however, to a similar conclusion. The syllables were learned in trochaic measure. The above mentioned investigators found that, of two syllables belonging to the same foot, the second one tended to revive the first.[2] They also concluded that in such cases we may explain the direction of succession not only as a backward one, but also as a tendency to revive the whole complex in its original serial order.[3] This gives us the first indication of any 'initial' tendency in ideal revival.

Now it is evident that with a trochaic measure, the question is still undecided as to whether such revival is backward or initial. In another series of experiments, Müller and Pilzecker presented evidence in favor of the latter tendency. Instead of using a trochaic beat, they used the anapestic foot. With three syllables, revival of the second upon presentation of the third, would help fasten the belief in a backward association, while revival of the first would increase our belief in an initial tendency in mental reproduction. The results of their experiments were in favor of the initial tendency.[4]

II.

In the series of tests given by the above mentioned investigators on the question of the initial tendency in serial revival, it seems to me conditions are, in this special case, somewhat artificial. In the first place, we do not, in the natural order of events, speak in nonsense syllables, with the accent on each odd or third one. Again, any possible connection and development of the serial meaning in each member is reduced almost to a minimum, and there remains in the series the tendency of the members to exist as discrete and separate units. Of course any rhythm or body attitude taken towards each member in the series would tend to give them a serial unity, and thus would tend to give them serial meaning in greater or less degree. Finally, it is rather difficult to obtain a pure series of syllables, *i. e.*, a set of syllables without any previous connections. In the various connections of syllables, it is possible that a syllable which precedes in the artificial series, may have been previously associated in the common course of events, as a succeeding syllable. It is necessary to have nonsense syllables if we use them as did Ebbinghaus and the others for the purpose of determining gain of time, etc., when learning them in different orders. But I think it hardly necessary to use nonsense syllables to determine whether the memory runs in a forward or a backward direction, or whether there exists an initial tendency or not. I may, of course, be wrong, but I think that if we wish to find out how a person remembers, we need but simply give him a cue, and see what comes into his mind after such cue, restrictions being placed as to the topic remembered. The natural order of mental succession can then be noted. Care, however, must be taken to get the very first ideas arising upon presentation of the cue, and precautions must also be taken to deal only with what is thoroughly known and is easy of recall. Where there is a halting process, I venture to suggest that this is not memory proper, but a species of perception mixed with memory. When a person has to struggle to get the next word in a series, each word, as it is dragged out, acts as a new stimulus, as an actual impression influencing further revival. In pure memory, the process ought to go off upon presentation of the stimulus alone. It ought to be more or less automatic.

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[1] *Ueber das Gedächtnis*, § 40.

[2] *Zeit. f. Psych.* 6 : 163.

[3] *Ibid.*, 6 : 309.

[4] *Ibid.*, *Ergänz. Band.* I, Ch. VI.

In collecting the data to determine the presence or absence of an initial tendency, I tested a class of boys in the sixth year (second half), about forty in number, averaging 12 years 5 months, the ages varying from 10 years 5 months to 15 years 5 months. The selection which I used was a poem which they had already memorized for regular recitation purposes, and which they 'knew' fluently. In the first five tests given below, I selected parts of this poem, no emphasis being placed upon the rhythm, the natural rhythm alone operating. The boys did not know the object of the test. I emphasized the following points before beginning: Upon my calling out a word, they were to write down the very first words of the part chosen which came into their minds. I dwelt upon this point, and also the fact that everything they put down was correct, so long as it was the first thing which arose, and so long as it belonged to the part of the selection chosen for the purposes of the test. In the first test I asked them to write down the first word, but in all those following, they wrote down the first three or four words arising upon presentation of the cue.

Before giving my results, it may be advisable distinctly to state the meaning of the terms used and their abbreviation. F=*fromward* tendency, *i. e.*, one going straight ahead upon presentation of the cue. For example, in the lines,

"The blessed damozel leaned out
From the gold bar of Heaven."

when given the cue, 'the,' I recalled 'the gold bar of Heaven,' such revival being *fromward* the cue, and in a *forward* direction. On the other hand, given the cue, 'leaned,' I recalled at once the series 'The blessed damozel leaned out.' This latter revival illustrates the *initial* tendency, which also goes in a *forward* direction and is designated by I. Both tendencies are in a *forward* direction, though the one is *fromward*, the other *initial*. When, in the tests given, no words were recalled, or where there was a halt due to sudden disturbance caused by the stimulus, or inability to recall, I have put the result under B or blank. In each result, I have stated the date when given, the part of the memory piece selected, the list of cues presented, the number of boys acting as subjects, and the number of cases, the last being distributed under the headings I, F, and B as above explained. Where a boy took up the cue and proceeded straight ahead, I have counted the case in the fromward column, and I have also put in the same column those cases in which revival immediately follows the cue, and in which there is an apparent 'skip' ahead of the cue before revival occurs. Certain possible objections to my method of procedure I shall take up after having presented my results.

TEST I.

Date. March 20, 1905 (forenoon).

Part selected.

"By the flow of the inland river."

Cues. *flow*, *inland*, *river*, *the.*

Number of boys. 38.

Cases.

I.	F.	B.	total.
86	65	1	152

TEST II.

Date. March 20, 1905 (afternoon).

Part selected.

"No more shall the war cry sever,
Or the winding rivers be red."

Cues. *sever*, *winding*, *rivers*, *shall.*
Number of boys. 40.

Cases.	I.	F.	B.	total.
	104	55	1	160

Test III.

Date. March 27, 1905 (afternoon).
Part selected.

> "By the flow of the inland river,
> Whence the fleets of iron have fled,
> Where the blades of the grave grass quiver,
> Asleep are the ranks of the dead,—
> Under the sod and the dew,
> Waiting the judgment day:
> Under the one, the Blue,
> Under the other, the Gray."

Cues. *fleets*, *dew*, *day*, *flow*, *under.*
Number of boys. 42.

Cases.	I.	F.	B.	total.
	128	78	4	210

Test IV.

Date. April 3, 1905 (afternoon).
Part selected, same as in test III.
Cues. *of*, *blades*, *asleep*, *under*, *the*, *of.*
Number of boys. 40.

Cases.	I.	F.	B.	total.
	56	182	2	240

It is interesting to note that in cues 1 and 6 (*of*) which are the same, a different series was revived for each in the cases of 25 boys, and that of these 25 boys, 10 differed in that one revival was fromward, while the other (for the same cue), was initial. The other 15 boys revived an entirely different series in the two cases for the same cue.

Test V.

Date. April 10 (afternoon).
Part selected,

> "Sadly, but not with upbraiding,
> The generous deed was done:
> In the storm of the years that are fading
> No braver battle was won,—
> Under the sod and the dew,
> Waiting the judgment day:
> Under the blossoms, the Blue;
> Under the garlands, the Gray."

Cues. *in*, *sod*, *fading*, *years*, *not*, *under*, *day*, *the*, *under*, *braver.*
Number of boys. 39.

Cases.	I.	F.	B.	total.
	120	269	1	390

In this test 14 boys revived, for the same cue (*under*), a different series in each case, *i. e.*, the same boy for the same cue gave a series differing in the second revival upon presentation of the cue.

In the following set of tests, about half the boys were the same as those acting as subjects in the preceding experiments. The class numbered about forty, was in the seventh year (first half), and averaged 12 years 3 months, the ages varying from 11 years 0 months to 15 years 9 months. Similar explanations were given as in the above tests.

TEST VI.

Date. October 16, 1905 (afternoon).

Part selected.

> "Whither, 'midst falling dew,
> While glow the heavens with the last steps of day,
> Far, through their rosy depths, dost thou pursue
> Thy solitary way?"

Cues. *glow, steps, way, with, the.*

Number of boys. 41.

Cases.	I.	F.	B.	total.
	64	133	8	205

The 8 blanks were all caused by the cue 'way,' which, it may be noted, is the last word in the stanza chosen. These eight cases might perhaps be classed under F, since the blank may have been caused by a natural inability to proceed onward.

TEST VII.

Date. October 24, 1905 (afternoon).

Part selected.

> "I chatter, chatter, as I flow
> To join the brimming river,
> For men may come and men may go,
> But I go on for ever."

Cues. *flow, river, men, I, forever.*

Number of boys. 36.

Cases.	I.	F.	B.	total.
	84	89	7	180

Similar to the cases in test VI, the seven blanks were caused by the cue 'forever' which is the last word in the stanza.

TEST VIII.

Date. October 25, 1905 (afternoon).

Part selected.

"The quality of mercy is not strain'd,
It droppeth as the gentle rain from heaven
Upon the place beneath: it is twice bless'd:
It blesseth him that gives and him that takes."

Cues. *quality*, *it*, *gentle*, *blesseth*, *the*.

Number of boys, 39,

Cases.	I.	F.	B.	total.
	19	170	6	195

The blank cases were caused by the cue 'the.' I infer from this that lack of revival was due to conflict, to shock due to the number of series which have equal right to follow 'the' and which would strive so to do.

TEST IX.

Date. October 26, 1905 (forenoon).

Part selected.

"We hold these truths to be self-evident, that all men are created equal, that they are endowed by their Creator with certain unalienable Rights, that among these are Life, Liberty and the pursuit of Happiness."

Cues. *truths*, *created*, *men*, *that*, *these*.

Number of boys. 37.

Cases.	I.	F.	B.	total.
	44	141	0	185

Before analyzing the above results, I think it advisable to answer a possible objection which may be raised. Since the selections were learned for purposes of class recitation, did not the boys therefore tend to recite the answers in the order in which they were usually required to do? Since they knew that only a certain method of recitation would be accepted, would not this influence their mode of reaction? If I wished to investigate the fromward tendency, perhaps such objection might have some weight. But the fact that a fromward direction is usually required in class recitation would operate strongly against any initial tendency becoming manifest. For a recitation proceeds onward, and not with a skip back and then ahead. The initial tendency, if shown (as it is), is strongly opposed by the artificial conditions imposed. This peculiar skip back and then the process ahead (initial tendency forward), moreover, cannot be explained by any backward revival. For if such were to operate, the intermediate words would have to be revived. This point I shall discuss more fully in the following section. Before, however, proceeding with my analysis of the results, I shall distribute the cases in Test VII, and give those examples of initial and fromward tendencies which occurred most frequently.

Test VII.
Cue. *flow.*
I cases. 16.
'*I chatter chatter as I flow*'
'*chatter chatter as I flow*'
'*as I flow*'
'*I flow*'
F cases. 20.
'*flow to join the brimming river*'
'*to join the brimming river*'

Cue. *river.*
I cases. 26.
'*to join the brimming river*'
'*the brimming river*'
'*brimming river*'
F cases 10.
'*river for men may come*'
'*for men may come*'

Cue. *men.*
I cases. 9.
'*for men may come*'
F cases. 27.
'*men may come*'
'*may come*'

Cue. *I.*
I cases, 4.
'*but I go on*'
F cases. 32.
'*I chatter chatter*'
'*chatter chatter as I flow*'
'*I go on forever*'
'*go on forever*'
'*flow to join*'

Cue. *forever.*
I cases. 29.
'*but I go on*'
'*I go on forever*'
'*go on forever*'
B cases. 7.

It is to be noted that no attention was paid to spelling or punctuation. It is also to be noted that the last word in the answer of some of the cases (as in the cue *forever*), is the same as the cue of the following test. It is, however, interesting to note that of the seven boys that put a blank when the fifth cue was called out (*forever*), five of them had 'forever' as the last word in their answers to the fourth cue. The answers above distributed are those which predominated. In addition were cases in which there were words or even whole lines skipped in the fromward cases, such skipping being, in the usual atomistic explanations, classed as 'mediate' association. Since I wish, among other things, to show the fallacy of the atomistic conception, I shall, in the following section, try to give a different explanation.

III.

Though our ideas or conscious moments come in serial order, this fact does not necessitate the existence of any potential series in the same order as that in revival. The fact that a series of ideas, etc., arises in the order,

$$a,\ b,\ c,\ d,\ \ldots\ldots\ p.$$

is no proof that stored up in our 'empty cabinets' is a number of ideas, *a*, *b*, *c*, *d*, etc., which drop out, as it were, one by one. The existence of an initial tendency in mental reproduction requires a far different conception of the present moment of consciousness. When written in symbolic form, as words, etc., the series seems to presuppose a number of discrete units which seem to exist in chain-like form in consciousness. But with such a series, how account for the initial tendency? In any series, *a*, *b*, *c*, *d*, etc., a jump back from *d* to *a* or to *b* will neglect, apparently, the intermediate member, *b*, and this vaulting process, a mental gymnastic, as it were, can hardly be explained by any 'mediate' association, backwards. For the series runs on ahead, forward, and not backward. A skip from *a* to *d might* be due to 'mediate' association, but 'mediate' association cannot be called in to explain the apparent jump backward. I shall try to give a rational explanation of both forward and backward 'jumps,' and shall attempt to show the futility and falsity of any explanation by 'mediate' association.

Now, instead of supposing the stimulus alone to exist as a conscious moment in the subject, in the light of the facts presented, we can add the tendency to revive a series in a forward direction, which tendency I represent by *n*. Instead of representing the present moment by *a*, or *b*, or *p*, therefore, it should be represented by *pn*, *p* giving us the stimulus or that portion of it operating, and *n* the tendency of the present moment to pass on in serial order. But in addition, I think it safe to add another factor, or rather to point out another factor which exists in the present moment of consciousness. The portion *p* of the present moment exists, not *in abstracto*, but in a series; as such, besides its own meaning, besides the meaning which it has in general, it has a specific meaning due to its position in the series; its meaning becomes narrowed, as it were, because of its serial connections. Thus, in the distribution of cases (See p. 248), and also in Tests III and IV, the same cue revived different series, and in some cases with the same boys. Thus (See p. 246) *I*, being in different series, had a different meaning according to the series in which it was revived. In some cases the series was '*I chatter, chatter, etc.*,' in others it was, '*but I go on forever.*' This meaning, both as such and as serial, I wish to represent by *m*. The entire present moment may therefore be represented by

$$pmn$$

m standing for the meaning both *per se* and as modified by the series, and *n* for the tendency to develop the series in which the stimulus exists. Expressing the fact that *pmn* may be in any place in the series, and may, therefore, explicate the series at different points, depending upon its position in the series, we have as the present moment,

$$pm_x n_y.$$

The present moment of consciousness cannot be *an isolated idea*, but is rather an entire disposition, such disposition being excited by the stimulus or cue, and having in it (1) a meaning and (2) a tendency to explicate the series implicit in it. This meaning and this tendency, moreover, have not dropped from the heavens above, but have been developed by a previous succession or successions. Such successions, instead of being represented by the discrete members,

$$a,\ b,\ c,\ d,\ \ldots p.$$

may rather be symbolized by the formula,

$$an,\ bmn_1.\ cm_1n_2,\ dm_2n_3,\ \ldots pm_xn_y.$$

What is revived by the cue or stimulus is not an idea, *in abstracto*, but rather an entire disposition, having a tendency to develop the series implicit in it. Now, given this present moment with its meaning and its tendency, it makes no difference what part of the series it begins to develop. It may begin the series, or it may start at some part of the series immediately following the stimulus. For the present moment in its entirety, $p\ m_x\ n_y$, *is the series in summation*, and as such may develop the series at any point which the attitude calls for, *There is no 'mediate' association either way*. It is only when we consider p alone that 'mediate' association *may* be considered. But when $p\ m_x\ n_y$, is taken as a whole, 'mediate' association must drop out, both as a problem and as a fact.

It is only this conception of the present moment of consciousness, I think, which can account for the initial tendency, and (though I mention this by the way) for the apparent 'mediate' association. When the cue is given, there is revived in the mind of the subject an entire disposition, which represents, by its meaning and its tendency, some series or part of a series which has been previously developed. This tendency in explicating the series begins at such point as is necessary fully to develop the present moment, to expand its meaning, to enable it to 'fit' in its position. Now it makes no difference, as I have said before, where the tendency becomes realized. It may operate in an initial direction forward, or in a fromward direction forward, and in the latter case it may even skip a member or a set of members in the series. Looking at the present moment from this point of view, instead of from the older atomistic standpoint, we can, I think, explain the initial tendency, and at the same time remove the problem of 'mediate' association. There is no actual skip either way. The series, or what there is of it, is *all there*, and exists implicitly in the present moment. When we examine the series, there seems to have been some mediation, perhaps. But as a moment of conscious life, such mediation did not exist. *The mediation exists in the logical analysis of the series by an observer* and is one of the many striking examples of the psychologist's fallacy. The whole series is present, and where and how it becomes explicated depends upon how much of it is roused, how closely it is organized, how much unity exists in it, and so on.

Before closing my account, I wish to make some minor observations, both psychological and otherwise. In arranging the cases, there was forced upon me the fact that with boys of ability above the average, with boys who show care, such ability being manifest in the tests given by good memory, thoroughness of preparation, etc., in such cases, the initial tendency was more frequent than with the rest of the boys. I do not mean to say that any *general* power was shown. Only, that where care or ability had resulted in better work as far as memorizing the selection was concerned, such care and ability having already been previously manifest, this care and ability in memory was concomitant with a very evident initial tendency, whereas the poorer scholars manifested a tendency in the fromward direction. In addition, it is seen that the earlier tests show a greater number of initial cases; and in these tests the piece was much more thoroughly and longer known than in the latter tests. It is, however, hardly fair to compare these tests, since it is evident that in many cases the position of the word and its meaning would determine largely whether the tendency was to be initial or fromward. There would have to be many more cases, and exact correlation before a more stable generalization could be made. But in general I should be perfectly willing to estimate the ability or care of the boys expended in the work, by the presence of the initial tendency. A rough and tentative classification could, I think, be made. This seems, moreover, to coincide with the conception of the present moment of consciousness above expounded. For it seems natural that where special effort or ability has been put forth, the result would be to establish a more closely organized disposition, a better developed meaning, a more complete organic whole. Such disposition would tend to realize itself from the beginning, and not in piecemeal fashion. I offer this as suggestion only, and not as a valid generalization.

Finally, the above results agree with numerous empirical observations in other directions. The above tests were restricted to verbal series with meaning. But similar results may be noticed in manual work and motor adjustments. For example, when a boy is told to correct a drawing, his tendency is to begin it again, and he shows this either by rubbing out the whole drawing, or by asking for another paper. Or, if he is building with blocks, and is not satisfied with some part, he will often throw down the whole structure and begin again. The spasm which a child exhibits when, in a rage, he tears up his paper because of some error which has been pointed out to him, should, I think, be treated as a realization of initial tendency (which has been checked perhaps by refusal), and not as a fit of the ancient Adam. So in reading, and in the other arts, the child's tendency to begin all over should not be checked, for it seems to be the natural working of his mind. Much wasted effort and much disagreeable work could be avoided if this initial tendency were properly respected. Any error made is not an error *per se*, but an error which destroys serial unity, serial meaning, and before further meaning can be developed, the whole series must, perhaps, be gone through *in toto* before progress is again possible. Hence the initial tendency. The error has no organic connection with what follows, and a new start must be made.

My tests were given to boys, and as such, these tests must be taken for what they are worth. I have found, however, that as far as I am concerned, my mind works in a similar manner. Even if what I have presented be not wholly accepted, I hope it will bring into proper light both the initial tendency and the fromward tendency, and the newer conception of the present moment of consciousness.

ON JUDGMENTS OF "LIKE" IN DISCRIMINATION EXPERIMENTS.

By Frank Angell.

The class of judgments, which we in a general way term "like," have had a checkered career in experimental psychology. Originally more or less explicitly included by Fechner among his doubtful judgments in the Method of Right and Wrong Cases, they gradually acquired more and more expositive significance until we find them forming a basis for a logarithmic law of sensory memory (Wolfe) and for establishing a theory of recognition. (Lehmann.)

And the ontogenetic development of this class of judgments with a kindly consistency often follows the phylogenetic: reagents who, at the beginning of a series of experiments, rarely and hesitatingly set down the symbol for a "like" judgment—often in the later part of the work, come to express their convictions of likeness with underscored emphasis.

The factors whose presence in consciousness may bring about a judgment of "like" are probably numerous and certainly multifarious, and among them is a variation in tendency, noted by the writer in a previous investigation (*Phil. Stud.*, XIX, 19–20) to judge certain differences as like according to the variety in differences in value between norm and variable in the other comparisons of a series.

The existence of such a tendency was noted by Bruno Kämpfe in his experimental testing of the Method of Right and Wrong Cases (*Phil. Stud.*, VII, 548), and later on, the tendency appeared as a "by-product" from judgments on weights according to the Method of Constant Difference in Martin and Müller's "Analysis" (S. 22).

The object of the present investigation is a further analysis of this tendency in the hope of throwing more light upon this ambiguous class of judgments.

The scheme of experimentation for such an investigation is simple: a number of judgments, sufficiently large to serve as a basis for generalizing, is taken according to the method of constant differences with relatively small differences between norm and variables; this series is followed by a second containing some of the differences between norm and variable represented in the first, together with other and larger differences. The second series is then compared with the first with regard to "like" judgments on pairs of stimuli common to the two series. If there are more "like" judgments in the second series than in the first the chances are that this has been brought about by the inductive influence of the larger differences; if in addition, the judgments of "like" are attended by a stronger conviction of likeness, the probabilities are very great that the effect is due to the presence of the larger differences. The apparatus used was the sound pendulum.[1]

The reagents sat, of course, with their backs turned towards the instrument and at a distance of about ten feet; they were wholly ignorant of the object of the experimentation and of the procedure. At the beginning of the work, the series with the small differences (A) alternated in the experimental hour with the series of large differences (B). Under these conditions, however, the reagents became aware of the transition from one kind of series to another, as was indicated by the confused nature of the results. Accordingly the remaining period of work was divided into halves, the first of which was taken with the judgments of small differences (A), and the second with the larger judgments (B). Under these conditions the reagents were not aware of the transition, and although in the course of the later series they became aware of greater ease in judging, they referred it, so far as they formulated their impressions at all, to practice, favorable disposition on a given day, or what not.

An hour's sitting gave a set of four series with ten judgments in a series. In two of these series the reagents were given full time to write down introspections; in the remaining two the pairs of stimuli succeeded one another rapidly without time for recording the result of any other introspection than the judgment. The object of this was partly to collect enough material for statistical conclusions, and partly to overcome the irregularities in methods of judging which constant introspection tends to bring about. The question to be answered was:—"How do differences in the scale of stimuli values affect judgments of "like"? and it might too easily come about that the native hue of comparison would be so sicklied o'er with the hue of introspection as to obscure the tendencies under investigation. At the same time the introspections in the alternate series might well serve as a check on the generalization from the figures. The order of the variable in a given series was first determined by chance—a fact with which the reagents were made acquainted; this order was reversed and repeated in subsequent series until it had passed through all the conventional time-order and serial order changes, after which a new order was likewise determined by chance. The reagents noted their judgments according to the categories of "Louder clearly;" "Louder;" "Like;" "Softer;" "Softer clearly" and "Doubtful," the last named comprising the very few cases where a difference was perceived with the undetermined direction. The judgments of "like" were further divided into positive and negative according as they were attended by a conviction of likeness or resulted from an inability to perceive a difference. This list of seven categories of judgment is more formidable in logical analysis than actuality; the judgments themselves are unitary processes and the act which classifies them in no wise interferes with the processes themselves, in such a way for example, as would the injunction to watch the accompanying imagery.

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[1] This instrument has its scale given in degrees. There is no reason why the readings should not give directly the fall-height of the pendulum bob.

Table I gives the general results in the two groups of experiments—group A with relatively small differences between norm and variables—group B with small and large differences. The average number of judgments for each of the four reagents is about 216 in group A, and 245 in group B. The difference in number of judgments in the two time orders is relatively small.

Taking now the number of judgments on the five variables common to the two groups, we find that group A (small differences) gives 147 "like" judgments in a total of 599 judgments of all kinds, or 24.5%, while group B gives 138 judgments of "like" in a total of 489 of all kinds, or a per cent. of 28.2. That is, the effect of the large differences mixed in with the small is to increase the proportional number of judgments of "like," even when working against the tendencies of practice. More clearly is this effect shown in the character of the "like" judgments; for in group A the underscored or clear judgments of "like" make up but 33.8% of the total number of "like" judgments, while in group B the corresponding proportion is 40.6%. That is, in the group containing the larger differences, the reagents delivered proportionately more "like" judgments and had proportionately clearer conviction of likeness. The excess of "like" judgments in the B group on the variables common to the two groups is, however, not large—not so large indeed as the writer expected, but the reason is not far to seek. In the case of one reagent the proportional number of like judgments was greater in the A group than in the B in the proportion of 33 to 30, while the number of underscored judgments was practically alike in the two groups. But seemingly in this case no less than in the other reagents the general trend of the series influenced the ways of judging. For

TABLE I.

Number and kind of Judgments for each Variable and both Time Orders. Four Reagents. Norm equals 1.

(*A*) *Small difference between Norm and Variables.*

Values of Variables.	0.7		0.8		0.9		1.0		1.1		1.2		1.4		1.6		Totals.	
Time Order.	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2
Louder Clearly	1	12	1	12	4	16	2	3	9	1	9	1	19	1	22	2	67	48
Louder	1	33	5	18	18	29	31	22	12	8	20	4	19	5	16	2	122	121
Like Clearly	1	2	4	3	5	9	10	11	2	4	1	2	0	3	0	2	23	36
Like	2	2	9	5	12	18	15	15	11	8	10	11	3	9	1	2	63	70
Doubtful	0	2	6	3	3	3	1	5	2	2	1	6	1	0	1	1	15	22
Softer	18	1	14	3	24	10	10	21	6	14	0	18	2	12	1	24	75	103
Softer Clearly	32	0	5	0	14	0	2	2	4	6	0	2	0	14	0	10	57	34
Sums	55	52	44	44	80	85	71	79	46	43	41	44	44	44	41	43	422	434
																	Total	856

(*B*) *Small and large differences between Norm and Variables.*

Values of Variables.	0.4		0.6		0.7		0.8		0.9		1.0		1.1		1.7		2.0		2.6		Totals.	
Time Order.	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2
Louder Clearly	0	44	0	22	0	12	0	4	2	5	0	0	0	1	29	0	30	0	42	1	103	89
Louder	0	6	0	26	5	29	6	24	9	38	5	4	23	6	16	1	16	3	3	1	83	138
Like Clearly	0	0	1	0	0	1	3	6	11	10	4	7	6	10	0	0	0	0	0	0	25	34
Like	0	0	1	1	0	3	7	7	22	13	4	3	10	11	1	1	0	0	0	0	45	39
Doubtful	0	0	1	0	2	2	5	4	10	7	1	3	0	5	3	1	0	0	0	0	22	22
Softer	5	0	13	0	23	0	26	3	24	8	0	5	8	14	2	27	1	26	0	7	102	90
Softer Clearly	46	0	32	0	19	0	4	0	2	0	0	1	0	2	0	15	0	22	0	41	103	81
Sums	51	50	48	49	49	47	51	48	80	81	14	23	47	49	51	45	47	51	45	50	483	493
																					Total	976

this reagent is what we might term of a positive or impulsive type of temperament; in the statements of his everyday conversation he rarely uses qualifying words, but adds them, if he deems it necessary, in a subsequent sentence. In the A series he underscored 60% of his judgments as against an average of 19% for the remaining three reagents. In the B series, however, with all its easily noted differences, his per cent. of underscored judgments was only 61, while in the case of the rest it rose from 19 to 36. In the case, therefore, of the impulsive reagent, the effect of the large differences was the reverse of what it was with the more deliberative type; starting out with a high degree of confidence in the correctness of his judgments this reagent was somewhat shaken in his confidence by the presence and obvious differences of the second series, so that he became more cautious and circumspect in his comparisons.

Reagent Co., on the other hand, is of a pronounced deliberative type; he is very careful in forming his opinions and cautious in expressing them. "It seems to me," "I think," or the deliverance of a judgment in the form of a question, are his usual ways in making statements. And as was to be expected, the effect of the course of the experimentation was the reverse of what it was on A. Not only did he have more "like" judgments in the B series than in the A, on differences common to the two series, but his proportion of judgments marked "clearly" was about eight times as great. (The actual figures are, A series, 1.8%; B series, 16%). That is, he not only had a greater tendency to judge "like" in the B series, but he had much greater confidence in his judgments, though he actually made more correct judgments of "like" in the series with the small differences, in the proportion of 28% to 16%.

Reagents Na. and Ya. come in between the extreme types Co. and Al. but approaching more closely Co. They represent the most common type among students.

We find here, therefore, two conditions affecting the number and correctness of judgments of "like:" 1° the Type: some individuals are more prone to express judgments of "like" than others, and this difference corresponds to the difference between deliberative and impulsive temperaments. This disposition, however, is not to be confounded with a tendency producing similar results with reagents who are not well-trained, viz., an inclination towards either judgments of "like" or of "unlike" as a result of a more or less vague notion of the general conditions of the experimentation. 2° The inductive effect of the course of the experimentation; series containing mainly large differences between norm and comparison call forth other judgments with given differences than those containing mainly small differences; the usual effect of the series with large differences is to increase the proportion of judgments of "like."

A third effect which has been clearly shown by previous investigations is that of time; the number and accuracy of judgments of "like" decrease with the time-interval between norm and comparison, though that this decrease always follows a logarithmic ratio, or that it is due to the "fading" of a memory image are in no wise matters of fact. The number of judgments of "like" fall off with the increase in time-interval because reagents maintain more constant bodily conditions in the short intervals than in the longer, and the accuracy falls off because there are fewer direct acts of comparison, though what the terms of these simple comparisons may be, to what category of sense they may belong, is not easy to determine. (*Vide* Hayden, Memory for Lifted Weights, *Am. Jour. Psy.*, XVII,

p. 497; Angell, Comparison of Shades of Gray, etc. *Phil. Stud.*, XIX, S. 20.)

In comparing simple stimuli like shades of gray or sounds, the usual process seems to be that reagents begin with negative judgments of "like;" that is, the judgment "like" is delivered because the reagents fail to note any difference between norm and comparison; and is not usually due at first to a conviction of likeness or a feeling of familiarity even after the reagents have begun to feel acquainted with the stimuli. Thus on the second day of the present investigation, a series of five comparison sounds were given without a corresponding norm, which the reagents were to compare with the norm used two days previously. At this time, although the reagents had made but 39 comparisons of sounds produced with the sound pendulum, these one term judgments were delivered in most cases quickly and often with considerable conviction of correctness. But in the subsequent judgments of this day there were no underscored judgments of "like;" that is, the judgments of "like" at this stage were still negative. It is at this stage, therefore, that these judgments are hardly distinguishable from the so-called doubtful judgments, and they further agree with the latter in taking a relatively long time in formation. In the course of time, however, this absence of a difference becomes a positive mark, so to speak, and such judgments are often delivered with more or less of a conviction of likeness. And along with this there grow up other positive signs which form bases for judgments of "like;" as the writer has before remarked (*op. cit.* S. 18.): "When a series of comparisons is made up of stimuli differing in part but a little, and in part not at all, from the norm stimulus, the judgment of "like" may be attended by a conviction of likeness which very often is due to the feeling, mood, tension sensation or even accidental circumstance."

In the present investigation the tension sensations were usually located in the ear; Al. notes repeatedly "muscular sensations in the ear," and Co. speaks of "reaction in ear." Resonance, referred sometimes to the external passages of the ear, sometimes to "echo" was frequently used as a basis for discrimination. In two cases, "a feeling of familiarity" with the norm formed one term of the comparisons, for Cl., the most experienced observer, and in another instance an organic sensation served the same purpose for Co. who notes that a 'thrill was experienced resulting from R_1 as significant, and this disturbance was held over till R_2 came.' The acoustic image of R_1 was not often noted, and when noted it did not seem to especially assist in discrimination. Cr. speaks of "A clear memory image but it did not help comparison." When held over for comparison, the acoustic image frequently took some other form than that of its prototype: Al. several times says that the "memory image" is continuous between R_1 and R_2 and this continuous form of reproduction of momentary stimuli, is frequent in sound and tone discrimination.

It is evident that the judgments gotten from experiments of this kind with discrimination of such simple stimuli as sound and shades of gray can enter only in part, and perhaps in small part, into the construction of a theory of recognition. In the first place, most of the judgments of "like" in the early stages of the investigation must be excluded as inferences. Of course it was only at the beginning, if at all, that these inferences approximated an explicit form, such as "I perceive no difference, the stimuli must be alike." They quickly became more and more syncopated and condensed until they existed as an abstract state of knowing (*Bewusstheit*)— abstracted, that is, from any background of imagery or sensation, acoustic, motor or otherwise. But that in this vicarious form, many of the judgments were still essentially inferences, and not direct recognitions is shown by the inductive effect of the "unlike" judgments which formed, so to speak, the major premise for the condensed inferences.

Even with the absolute, or free judgments we have to do rather with cases of cognition or at best of class recognition than with the recognition of a specific impression. The experiment of the second day of this investigation (*vide* p. 258) when, after having delivered but 39 judgments on these sounds, the observers were able to compare stimuli with a norm given two days previously, shows how quickly the stimuli were classified.

It is further to be observed that in discrimination experiments where a definite plan of response to stimuli is set before the observers, a state of affairs ensues which is very unlike the ordinary conditions of recognition. There is, for example, a greater difference between discrimination of tones and the "absolute" recognition of a tone than lies in the relative number of terms of comparison. In his article, "Ueber das absolute Gehör" (*Z. f. Psy.*, III, S. 267) J. v. Kries writes that "In the first place it can come to pass that the hearing of a tone can produce in me immediately a certain name, say *c*, but nevertheless, I am left in a state of doubt as to whether I heard *c* or *d*. On the other hand, recognition is not excluded by the fact that in hearing a tone I feel no immediate impulse to give it a definite name." This is a very different mental condition from that of which Ach gives an account in his reaction experiments in discrimination. (Ueber die Willenstätigkeit und das Denken, S. 88.) He says, "After the sensation had developed, especially in the case of the colored cards, the name of the color in question, red or yellow, was mentally spoken with a certain affirmation or assent, after which the finger came up. In other cases blue or yellow was apprehended as colored according to the language of the instructions and after this the motion followed."

In the present investigation, the decision on the comparative loudness of the sounds, became practically a reaction or response to the second sound in terms of "louder," "softer," or "like;" these terms or some abstract equivalent for them were 'nascent,' to use a Spencerianism, and when the comparison sounded it was immediately thought of as under one of the catagories.[1] Occasionally it was classified by means of some sensory sign,—resonance for example, but usually the classification resulted without sensory or ideative basis.

How far beyond the conventional scheme of comparison these processes had gone is shown in the case of Observer Cr., who classified the sounds as unlike on the basis of a feeling of familiarity belonging to the norm—the recognition of one sound thus forming the criterion of a differerence. But the judgments of "like" in discrimination experiments with simple stimuli seem to be rarely recognitive; they are most usually based on inference or on cognitive classification, and when recognitive they are more apt to arise from imagery and sensations that are accidental than from the stimuli forming the direct objects of discrimination.

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[1] *Cf.* Whipple, Discrimination of Clang3 and Tones (*Am. Jour. Psy.*, 12, p. 445)

PSYCHOLOGICAL LITERATURE.

Instituts Solvay. Travaux de Sociologie. Notes et Mémoires. Misch et Thron, Éditeurs. Bruxelles et Leipzig, 1906. *Fascicule 1. Note sur des Formules d'Introduction à l'Énergétique physio- et psycho-sociologique.* Par E. SOLVAY. pp. 26. *Fascicule 2. Esquisse d'une Sociologie*, par ÉMILE WAXWEILER. pp. 306. *Fascicule 3. Les Origines Naturelles de la Propriété. Essai de Sociologie Comparée.* Par R. PETRUCCI. pp. xvi+246. *Fascicule 4. Sur quelques Erreurs de Méthode dans l'Étude de l'Homme Primitif. Notes Critiques.* Par LOUIS WODON. pp. 37. *Fascicule 5. L'Aryen et l'Anthroposociologie. Étude Critique.* Par le DR. ÉMILE HOUZÉ. pp. 117. *Fascicule 6. Mesure des Capacités Intellectuelle et Énergétique. Notes d'Analyse statistique.* Par CHARLES HENRY, *avec Remarque additionnelle* (*Sur l'Interprétation Sociologique de la Distribution des Salaires*) par EMILE WAXWEILER. pp. 77.

The number and extent of its publications are an indication of the activity of the recently established "Instituts Solvay," one of the scientific foundations of the Belgian capital. The first monograph, by the founder himself, is an attempt to condense in two mathematical formulæ the physiological expression, on the one hand, and the psychological expression (of organic reactions) on the other, of the life of any given isolated individual, and to indicate the modifications of these formulæ necesssary to adapt them to the case of an individual no longer isolated by living in social relations. Society is something more than the sum of a number of individuals and the productive intellectual capacity of each individual is an important interventional factor. The energy of a social group is likewise not newly and purely the sum of the utilizable individual energies of the people composing it. The goal of M. Solvay's sociology is "to reduce to fundamental physico-chemical actions, the *ensemble* of biological and sociological phenomena."

In his "Outlines of Sociology," Emile Waxweiler, who is a professor of the University of Brussels, treats, in the first part, of sociology (adaptation to environment, living *milieu* and social *milieu*, sociological phenomena in comparative sociology) and, in the second, sociological analysis (sources and method, social formation, social aptitudes, activities and synergies). Professor Waxweiler defines "social ethology," or "sociology," since that term already exists, as "the science, or rather, the physiology of the reactional phenomena due to the mutual excitations of individuals of the same species without distinction of sex." The basis of social affinity is the "impression of organic likeness (similitude)," and the evolution of man's nervous system has determined characteristic phenomena from the sociological point of view,—"the faculty of perceiving inter-individually specific likeness of organization proceeds on a par with what is called the manifestations of intelligence, *i. e.*, with the complexity of the nervous system" (p. 74). More and more has man become "the animal formed by the other individuals of his species." The author styles *primitive* "those men, who, with regard to the greatest mesological complexity attained at a given epoch, have remained at an elementary stage of the development of their sensibility (or, in other words, their reactional potentiality)," and *civilized*, "those who have put this potentiality in unison with the variety of environmental stimuli." *Sociability*, "the human aptitude for social impression," belongs properly to man alone. Prof. Waxweiler accepts the recapitulation theory and holds that "the peoples who have remained close to the initial moment of phyletic evolution are more like the child of the civilized man than like the civilized man himself" (p. 112). The "social personality" of an individual consists of three elements,—"an *ensemble* of physical adaptations, an *ensemble* of psychic adaptations, and a sort of "mental plant" (or stock of tools, etc.),—this might better, perhaps, be termed *idiotropism* than *social personality*. When the individual has completed his education (in *sociability* especially) he has acquired a certain representation of the other individuals of his species, a representation which is "essentially polymorphic" (p. 136). The phenomena of mental parallelism (telepathy) rest upon social synethia, leading to coincidence of intuitions. In accordance with the development of man's sensibility and the increase of the social polymorphism due to the intensification of culture, the desire of the like evolves towards a form implying now only occasional and quite elective *rapprochements*. The most complete human type today is, that of the "individual who 'finds himself again' in the least part in a large number of his fellows, towards whom he feels himself drawn by lively impulses, but who does not 'find himself again' completely, or even, in large part, in any one." The only activities of the individual which interest the sociologist are his external activities, and those only in so far as they "produce effectively in another individual of the same species, without distinction of sex, a certain reaction" (p. 169). Activities are distinguished as conjunctive, protective, injurious, competitive, divulgative, gregarious, repetitive, initiative, acquisitive, selective; the social synergies a conformity, interdependence, cephalization, co-ordination, conscience, etc. There is much interesting matter in this volume and the bibliography (pages 297-306, 2 cols. to page) proves the author's wide reading,—he has made good use of the *Pedagogical Seminary* and the writings of American devotees of "child study." But for all this his book is, as he terms it, properly enough, "a sketch." A useful feature is the "sociological dictionary (pages 281-295) containing some 2,200 terms without definitions, of more or less sociological import, gleaned from the vocabulary of the French language.

R. Petrucci's essay in comparative psychology, "The Natural Origins of Property," treats in large measure of the idea of property in the infra-human world of life (pages 1-77) and then, less extensively, of its first developments in man. The earliest form of property is the prey obtained by the unicellular organism by reason of the needs of nutrition. At this biological stage we meet also the permanent or transitory colonial organization of unicellular animals, on which is grafted a sort of primitive form of property of the "family type." Even in the vegetable world, however paradoxical it may seem at first, an analogous state of affairs exists, the possession of the soil by plants individually and collectively constituting "property" of the two kinds in question. The development of these forms of property is traced by the author in the molluscs and worms, insects, arachnids, crustacea, fishes, reptiles and batrachians, birds and mammifers and man (only the primitive hunting and pastoral stages being considered, as they present the simplest forms of human property, and the discussion of the juridical forms of property lies outside the scope of the work). The contrast between the crushing collectivism of certain insects and the greater significance of the individual and the family among the vertebrates is emphasized. In the birds are to be seen the forerunners of individual, family and group property, the further development of which characterizes the mammals. The chapter on "The First Forms of Property in Man" (pages 179-218) is not very satisfactory, as the list of authorities cited (pp. xiv-xv) would almost necessarily imply. Before an exact statement of the facts of property among primitive peoples can be made, certainly the records of the investigations of the experts of the Bureau of Ethnology, the Jesup North Pacific Expedition and other equally valuable studies concerning some of the least civilized and most primitive people, of the world, must be thoroughly digested. Such researches as that of Dr. Jenks on the wild-rice and its use by the Indians of North America, Boas' brief account of property-marks among the Alaskan Eskimo, etc., are significant here. The relation of "bachelors' houses," puberty-cabins and other isolation places for men and women at various periods of life to the general institutions of primitive man needs more careful study; also the relation between puberty, etc., and art-production as connected with property-sense. According to Petrucci, the uncivilized peoples exhibit essential characteristics already seen in the animals below man: food-reserve, hunting-ground, possession of shelter, all these are grouped according to the same individual, familial, collective forms. With man individual property appears in weapons, tools and clothing, while the family-side of property is found in the shelter (dwelling house) and the collective or group aspect in the hunting ground and territory exploited, but these distinctions have no impassible limits. The greatest element of perfectibility possessed by primitive man, according to the author, is "the weapon and the tool, which, in the beginning are one and the same." The permanent utilization and the improvement of weapons and tools and the adaptation of clothing as a sort of defense against the environment, sparing at the same time his organic activity, are some of the chief advantages of man over the other animals. Man's hand and its "prolongation" in the tool and weapon made primitive property a fact. Things that are merely transient with the animals have become fixed and permanent with man. Among other things, Petrucci holds that the family is not a social unit, but "a formation *per se*," that social evolution is not bound up with organic evolution, nor is the social phenomenon connected directly with intellectual evolution. In a chart-appendix is given a conspectus of the "comparative sociology of the phenomena of property."

Professor Wodon's "Errors of Method in the Study of Primitive Man" is an incomplete and rather unsuccessful effort to controvert the principal theses in Bücher's *Die Entstehung der Wirtschaft* and *Arbeit und Rhythmus*. The views of Bücher, which are particularly antagonized, are the general theory of the non-economic and non-industrial character of the condition of primitive man (Wodon styles it "economic chaos);" the doctrine of the economic separation of the sexes (Wodon maintains, *e. g.*, that, in the case of the Indians of the Xingu in Brazil, where sex-distinctions in labor are reported to exist, the distribution of work between the sexes is really in correlation with their natural aptitudes; the division of labor in vogue here has nothing in common with the "isolation" of Bücher); and the theory that work has developed out of non-work, play and art having existed before the serious activities to which the name of "work" may be given, while, rhythm has been one of the principal economic factors of evolution. In combatting Bücher, Wodon makes good use of the material in Grosse's *Die Anfänge der Kunst* and *Die Formen der Familie*. According to the author, Bücher's primitive man is "a mere phantom.

less real even than the *homo oeconomicus* of the classic economists," But this contention needs much more proof than is vouchsafed in this small monograph.

The articles of C. Henry and the "*Note*" of E. Waxweiler are mathematical studies of the sociological problem of the measurement of the intellectual and "energetic" capacities of a given collectivity. The three papers of Henry treat respectively of "the criterium of irreducibility of statistical *ensembles*." "The decomposition of pseudo-binomial curves into binomial curves," and "cotes et mesures." Dr. E. Houzé, the author of the monograph on "The Aryan and Anthropo-sociology," is Professor of Anthropology at the University of Brussels, and his aim is to show that the "so-called Aryan" is "not a primitive people, but an invention of the study-room," and that "anthropo-sociology" and its alleged "laws" are based on "fundamental errors, statistical, anatomical, physiological, psychological and historical." In the first part of his study Professor Houzé discusses the "Aryan" from the linguistic, historical, archæological and anthropological points of view, reaching the conclusion that, after all "there is no *Aryan* question," and that it is absurd to recognize among the various peoples of Europe "*one* human type superior to all others and *the* factor in all civilizations." The morphological Aryan does not exist; never has existed; and archæologic search for him is utterly vain. Aryan linguistics is a deceitful thing and has been responsible for many vagaries of "science." Europe has been the scene of the development of interesting forms of human culture, but their inspiration has not been chiefly "Aryan" nor Asiatic. The second part of the monograph is devoted to "anthropology," and in it the author points out that just as all civilizations have been produced by peoples and not by special types, the value of intelligence can never be revealed by examinations of human skulls;—all theories seeking to make psycho-physiological deductions from craniometry are necessarily false. The "pseudo-anthropology" of Lapouge and his school receives special attention in the third section, on "anthropo-sociology," which is styled a futile attempt to mix two distinct sciences. This book is interesting reading for those among ourselves who have added to the "Aryan" by imagining an "Anglo-Saxon" as the goal of his complete development. One is as non-existent as the other, if we believe Houzé. ALEXANDER F. CHAMBERLAIN.

L'Agrandissement et la Proximité Apparentes de la Lune à L'Horizon. Ed. Claparède. Archives de Psychologie, 1906, V, 121-146.

Preliminary to presenting his own views Claparède reviews previous theories put forward to account for the fact that the moon appears larger at the horizon than at the zenith.

The theories he discusses and puts aside as false or inadequate are refraction, pupilary dilation, fall of the crystalline lens, comparison, contrast, direction of the glance, overestimation of angles, weakness of peripheral vision, further distance at horizon. The last theory seems to him the most tenable and he explains it in some detail. It depends on the well established law of vision that for a given retinal image the object corresponding to that image seems larger where it is judged more distant. The moon is judged further away at the horizon and hence is seen larger. But against this, Claparède brings forward a series of experiments made by himself, and others by Zoth, showing that to 120 out of 125 persons the moon appears not further away at the horizon but much nearer.

In order to hold the classic explanation and at the same time account for the fact as established by Claparède's experiments that the moon at the horizon appears nearer, we must suppose that two contradictory judgments can take place in the same mind at the same time:—first, the moon at the horizon is more distant and therefore is seen larger, and second, the moon is larger and therefore nearer.

By means of an experiment with stereoscopic images obtained by convergence and divergence, Claparède found that the image of divergence which is judged further away and therefore seen larger, at times appeared nearer than the image of convergence, thus proving the possibility of two contradictory judgments taking place at the same time.

But even though this be so, the hypothesis seems improbable and Claparède is led to look for the cause of the illusion in another sphere, in the region of affection rather than of mere perception. Our affective attitude in perceiving the moon at the horizon differs from that in perceiving the moon at the zenith. In the first case we regard the moon as a terrestrial object either because it is not at first recognized or because it is in the terrestrial zone. Now objects in the terrestrial zone interest us more than objects in the sky. This change in importance we translate into a change in size. Because the moon attracts more attention at the horizon, we see it larger.

To establish this factor, Claparède experimented with a picture in which two moons appear, one at the horizon, the other at the zenith, and caused each to disappear in turn by means of a small black disk put over it. Fourteen out of twenty persons who observed the picture got the illusion of the moon at the horizon as larger. Here neither distance, form of the sky, direction of the glance could play the least rôle. The affective factor must have been the only one operative to cause the illusion.

But fourteen affirmative judgments out of twenty is not sufficient to establish any conclusion. The outcome of the paper has been simply to leave the problem richer by two unproved hypotheses.

Bryn Mawr College. V. ROBINSON.

Die Schätzung von Bewegungsgrössen bei Voderarmbwegungen. ROSWELL P. ANGIER. Zeits. f. Psy., 1905, Vol. XXXIX, pp. 429-447.

The movements studied were of the elbow joint only, with the arm moving horizontally. The movements compared were always in the same direction and between objective limits. The distances varied between 9.2 cm. and 10.8 cm. with one of the two distances compared always 10 cm. Angier was the only subject. Several series of experiments were made (always with closed eyes) to test the influence on the discrimination sensibility of differences in rapidity, in resistance, in position of the starting points of the movements compared and also to compare the precision of judgment in active and in passive movements.

His results are summarized as follows: The precision with which length of movement is estimated (at least in the case of the experimenter) is affected neither by resistance nor by change in the position of the arm (provided new muscles be not brought into play), nor yet by the passivity of the movement. A constant error is, however, introduced when the velocity of the comparison movement is made greater than that of the standard. In that case, the comparison movement is overestimated in 92% of the wrong judgments. For passive movements the proportion rises to 94%. In these comparisons the standard movement was made at a rate called 'natural' by the author.

The conclusions of previous experimenters agree with these, or at least do not contradict them, with regard to resistance, position and passivity. On the question of velocity, however, Delabarre and Loeb reached directly opposite conclusions. They found that quickly made movements were underestimated. Falk affirmed that the velocity was without influence on the judgment of length.

In attempting to account for the constant error due to velocity, Angier remarks that it cannot be due to the stronger muscular contraction which takes place in swifter movements, since the same error appears when the movements are passive. He finds the source of the error in the effect of the greater momentum of the rapidly moving arm. On coming in contact with the limiting objects, a sort of rebound takes place, the outcome of which is an 'irradiation of stimulus' in the joint, hence the overestimation. But, however one may account for the velocity-results of Angier, his experiments as well as those of his predecessors (Bloch, Kramer Moskiewicz, and Falk), confirm the conclusion of Goldscheider that the judgment of length of movement depends essentially upon joint-sensations.

The investigations of Angier and of his predecessors leave much to be desired on the score of accuracy in the determination of the velocity. Angier used a metronome to time himself; the others apparently did the best they could without the help of any apparatus. Further progress in the study of constant velocity-errors will depend, it seems, upon a method which will make possible exact measurement of the rate of motion and upon a wider range of experimentation with regard to length and direction of the movements compared.

Bryn Mawr College. JAMES H. LEUBA.

Sex and Society, studies in the social psychology of sex. By W. I. THOMAS. University of Chicago Press, 1907. pp. 325.

The nine papers here printed are essentially disconnected, having all been previously printed in journals, most of them in the American Journal of Sociology. They are of extremely different degrees of merit. In the chapter on the psychology of modesty and clothing, the author shows a regretable ignorance of the best literature on the subject, although he makes one or two interesting and novel suggestions. The adventurous character of women is made up of certain reflections of the author and is the most original chapter in the book. In the mind of man and the lower races again the author shows strange ignorance of the best literature upon this subject, although some of his suggestions are fresh. The chapters on the relations between sex, primitive industry and morality are interesting compends. As a whole the author shows a unique combination of freshness of thought and partial scholarship that is rather characteristic of what is popularly thought to the Western type of mind. Here, he glides smoothly over waters, the depth of which he does not dream and his remarks are trite and commonplace in the extreme. A few pages later, he drops suggestions that are well calculated to stimulate new thought. The author probably made no effort to cover in any systematic way the ground designated by his title. The critic is therefore somewhat baffled between a desire to congratulate and praise and the sense that he ought to censure. The title of the book is suggestive of far more than it contains and this leaves the writer and the publisher somewhat open to the suspicion of being unduly anxious to produce something that would sell.

Die Abstammung des Menschen und die Begingungen seiner Entwickelung Für Naturforscher Àrtze und gebildete Laien dargestellt von DR. MORITŻ ALSBERG. T. G. Fisher, Cassel, 1902. pp. 248.

The writer seeks to show that there can be no doubt of a former diluvial man of low culture, quite distinct from modern races. Man is not at the head of the animal kingdom in all parts of his organization, nor is the pithecanthropus the direct predecessor of modern man. The development of the latter can be traced to a relatively low developmental stage of mammalian life. Man is a branch of a tree which grew to considerable height without branches. Migrations of primitive men probably gave the first impulse to the oldest racial types. The smaller stature of women has nothing to do with the different social positions in the past.

The Biology, Physiology and Sociology of Reproduction; also Sexual Hygiene with special reference to the male. By WINFIELD S. HALL, 1906. Herbert A. Ray, Chicago, 1906. pp. 138.

The author treats reproduction, essentially from the standpoint of biology and gives an introductory chapter treating of egoistic and phyletic activities and sacrifices and compensation in both these fields. The rearing of young always involves sacrifice and should be met consciously in the interests of the race. The second chapter describes the physical and psychical changes connected with adolescence. The third is devoted to the anatomy and physiology of the male organs and the last to the sexual hygiene of the adolescent male with extremely plain talk upon illicit intercourse, sexual diseases, continence, diet, baths, exercise, sleep and the control of these. In an interesting appendix, typical questions selected from those actually asked the lecturer by young men are answered. The book certainly has the merits of brevity and of plainness.

Der Mensch und seine Tracht ihrem Wesen nach geschildert, von FRITZ RUMPF. Mit 29 Tafeln. Alfred Schall, Berlin, 1905. pp. 330.

This work is of very great value. The author has gone very deeply into his subject and the few dozen cuts at the end of the book are well chosen from a mass of possible selections so vast as to make choice hard. Our chief criticism of the work is that it is over-systematized. For instance, his main divisions are costumes for pilgrims, soldiers, vocations and society. Pilgrim's costumes are classified as for warmth, for coolness, for dryness and for health. Soldiers' costumes are for flight, defence, capture, battle, striking, thrusting. The utility garbs are for hunting, herding, building, travel, uniformity. The social costumes are those that distinguish sex, race, rank, associations, etc. Among the supplementary costumes are those that appeal to the ear, nose, taste, touch. The author's historic studies have been extensive and careful. The work is written rather more from an anthropological than from a philosophical or psychological standpoint.

Die Schöpfungstage, Umrisse zu einer Entwickelungsgeschichte der Natur. Von WILHELM BÖLSCHE, mit zehn Bildern nach Originalzeichnungen von Heinrich Harder. Carl Reissner, Dresden, 1906. pp. 88.

Within the last few years we have had a number of interesting attempts to present a brief outline of evolution to school children. This seems to the writer the most successful of the few dozen or so that have yet appeared and that have come within his knowledge, but nevertheless, to be still unsatisfactory. It ought to come fully down to man and to our thinking to be even more fully illustrated than by the ten full page cuts here found. This field is full of a kind of looming mystic magnitude and hence we can think of no domain in which the scientific imagination both for artist and writer should have freer scope. It is this aspect of the work that seems to us chiefly lacking in all the booklets of this class and it is toward this general characterization that evolutionary principles are slowly progressing.

The Law of Suggestion, a compendium for the people. By REV. STANLEY LEFEVRE KREBS. Science Press, Chicago, 1906. pp. 157.

This little book sketches the ancient history of suggestion with recent experiments and describes methods of hypnotizing and of administering suggestion, the use of indirect, positive and negative suggestion, refutes objections, describes dangers and in the last chapter describes some extraordinary phenomena. The great law is iteration, a law known to advertisers and moralists. By insisting in season and out of season upon the right precepts, conduct tends to work out character along the same lines. Most dangers are imaginary. The author gives four rules for the practice of self-control. They are to sit still, stand still, look still and be still for two minutes each.

Essai sur les Passions, par TH. RIBOT. Felix Alcan, editeur, Paris, 1907. pp. 192.

This volume is written with all the lucidity, comprehension and incisiveness of the author at his best. He first discusses at considerable extent what passion really is. Here our only criticism is that he has far too little to say of its phyletic origin which to our mind is one of the chief problems. Then in the second and third chapters on the genealogy of passion he seems to the writer to come little in contact with the rich and fruitful suggestions that arise for either the paleontologic or the psycho-genetic field. He has evidently paid more attention to abnormalities than to the study of normal development of passions in children. The fourth chapter is a unique specimen of a discussion which while extremely interesting, is yet extremely schematic. In answer to the question how passions expend themselves, they end in one of five ways: either by exhaustion, by transformation, by substitution, in insanity or in death. He very well remarks that people of intensely passionate nature are as rare as geniuses.

Les Substituts de l'Ame dans la Psychologie moderne, par NICOLAS KOSTYLEFF. Felix Alcan, Paris, 1906. pp. 228.

This somewhat startling title hardly justifies in the end the curiosity which it excites. The first of these substitutes is the chemical concepts of soul according to which consciousness is explained as the summation of epiphenomena. Under the head "Mechanical Conceptions of Life," the author has treated at length only Zender and has little to say of the larger school of which he is a member. In the third part, he undertakes to criticise psychical deliverances as they are conceived by Hering, Wahle, Mach, Avenarius, Ostwald and Laswitz. Of all these he is most attracted to the view of Mach. In the fourth part, he attempts to co-ordinate psychic deliverances with those of objective science treating of the mechanism of memory, mental images, abstract ideas, the ego, etc.

Mental Development in the Child and the Race. Methods and Processes. By JAMES MARK BALDWIN. With 17 figures and 19 tables. The MacMillan Co., New York, 1906. pp. 477.

This book reaches its third edition in celebrating its full decade. The author leaves it essentially as originally written, the revision being mainly in matter of details and exactness of exposition. He also announces a significant volume on the principles of genetic science in which the ideas of his series will be thrown together into concise and reasoned form. In this volume that is here announced, the outcome of the whole endeavor will be estimated and set forth in relation to the literature of several sciences to which these earlier books respectively relate. We forbear here to undertake criticism of this author's work until the appearance of this later volume.

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THE PSYCHOLOGY OF CHESS AND OF LEARNING TO PLAY IT.

By ALFRED A. CLEVELAND.

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In this study an attempt is made to sketch the psychology of the game of chess, to trace the stages in the development of a chess player, and to interpret this progress in psychological terms. That the task, owing to the complexity of the processes involved and the impossibility of applying anything like satisfactory objective tests, is a difficult one, is obvious, but it is one that seems to the writer worth attempting.[1]

I. THE PSYCHOLOGY OF THE GAME.

Chess is, as every one knows, a mimic battle fought upon a field of sixty-four squares with pieces moved according to an elaborate system and having powers suggestive of a variety of fighting units. The purpose of each player is to checkmate his opponent, that is, to hem in and threaten the latter's king in such a fashion that he is subject to capture at the next move. In our discussion of the psychology of the game it will be convenient to consider it first as a form of human play and then to take up more particularly the mental powers involved.

1. *Chess as a Form of Human Play.*

Forms and Varieties of the Game. The game of chess has not been confined to any particular age, race, country, or class. It is without doubt one of the oldest, if not the oldest, of the intellectual pastimes, and it is the game of skill *par excellence*. Its origin is not definitely known and there have been many claimants for the honor of its invention.[2]

Especially in its later history the game has developed a number of off-shoots in specialties which for many people share the interest of play across the board. The chief of these is the composition and solving of chess problems, which now has quite a literature and many devotees. Another is correspondence play, in which the strict rules of the typical game are somewhat relaxed on account of the peculiar conditions of play. Others, practiced as feats, but of especial psychological interest, are blindfold playing, to which Binet has devoted a special research,[3] and the playing of many games at once (either blindfolded or with sight of the board). To some of these special forms we shall return later.

It is a pleasure to acknowledge my indebtedness to Professor E. C. Sanford for the suggestion of this topic of study, and for generous assistance in following it out; to Dr. C. A. Drew and others at the Massachusetts State Farm for courtesies extended; to those who in the capacity of assistants have contributed much to this study; and to all who have answered my questionnaire on chess or assisted in securing answers to it.

[2] The history of chess may be followed in Forbes's History of Chess, and in Dr. Van der Linde's book on its history and literature.

[3] Binet, Alfred: Psychologie des grands Calculateurs et Joueurs d'Echecs, Deuxième Partie, Paris, 1894.

Instinctive Factors in Chess Playing. Chess is, as we have said, a game of wide distribution and popularity. Dr. Emanuel Lasker states that over one million English speaking people know the game, that there are in the United States, England and Canada between seven and eight hundred good sized chess clubs, many of which have over one hundred members each, and that the City of London Club has over four hundred members;[1] and judging from the number of chess clubs, chess periodicals and players of high rank in Germany, France, Russia, Austria, and Poland chess is no less popular in those countries. If one were asked what class or classes of people play chess one might truthfully reply that all classes play it.

The question then arises: Why has chess proved so widely popular at all times and in all places? How has it been possible for a game making severe intellectual demands to hold a place historically and in geographical distribution beside such universal forms of human play as gambling, horse-racing, athletics, and hunting, and to claim devotees, if less numerous, at any rate as loyal as any of these? The answer is, of course, that, in common with a multitude of other games and sports, it appeals to the fundamental instinct of combat, in a way that is direct and at the same time exempt from the anti-social features that are inherent in actual physical combat. Here lies a large share of its attractiveness, and its capacity for stirring emotion. It takes hold upon those suppressed survivals of savage impulse (if we are to credit the savage alone with a first hand liking for a contest) which in their modified exercise have been shown to be so large a factor in adult sport.[2]

In this, however, it shows but the typical qualities of the genus to which it belongs—that it is one of the strongly competitive games.[3] Its own specific attractiveness lies in the fact that it is a competitive game of *skill*, more particularly of intellectual skill as opposed to merely manual or bodily dexterity; it is a contest of scheme against scheme; it is a game of generalship.[4] Each particular situation appeals to the player, not only as an occasion for attack or defense, but also as a situation to be met by taking thought, a difficulty to be seen through and overcome, a problem to be solved. There is, therefore, in chess playing all the challenge that lies in baffling but fascinating problems and much of that which lies in the solution of puzzles. That the interest in this aspect of chess is real and important is abundantly evidenced by the growth of the chess

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[1] Lasker's *Chess Magazine*, Vol. I, No. 1, Nov. 1904. p. 48.

[2] Patrick: The Psychology of Football. *Am. Jour. Psy.*, Vol. XIV, 1903, pp. 368-381.

[3] Groos: The Play of Man, New York, 1901. pp. 173 ff.

[4] Groos: *op. cit.*, p. 190.

"problem," of which we shall have more to say presently. Lindley in his "Study of Puzzles"[1] holds it likely that in the puzzle solving passion we have a form of the preparatory play impulse to which Groos rightly attributes so much of both animal and human play.[2]

Still another factor of interest in chess is the pleasure of invention and origination, the pleasure of being a cause.[3] In the returns of my correspondents a decided preference is expressed for original plans of attack and defense.[4] Most say that they get away from the standard book plays as soon as possible after the first few moves. Some say that they play from book not from choice, but from necessity; but most say that while they follow the book openings for a few moves, they prefer to get away from them as soon as they possibly can without detriment to their game. They prefer their own game because it is more real, and is a better representative of their own ideas. As one player puts it, "There is little satisfaction in catching your opponent on a line of play that you have simply memorized." There are, also, of course, various practical reasons for this preference. An original plan throws both players on their merits and removes the game at once, so far as possible, from a mere memory exercise, thus depriving a player of the advantage which a superior memory or a better knowledge of book games might give. There is an advantage to the player himself in an original plan in that his game is more likely to be a unit and consequently more consistently played than one partly remembered and partly originated. While the inability to remember particular lines of play is undoubtedly a determining factor in the choice between an original plan and what is known as book play, nevertheless, there is something attractive about a game which one feels to be his own, especially if it is successful.

In summary we may say of chess as a form of human play that in the first place it is a contest, and, as such, it appeals to the fundamental fighting instinct, the instinct which in every normal individual impels him to measure his skill with that of others. In the second place chess offers its devotees opportunity to exercise their ingenuity in the solution of problems and puzzles, a form of pleasure that may well rest upon that general interest in the unknown which at one time must have had the greatest survival value. It would seem further, that intellectual activity is indulged in for the pleasure which such activity gives in itself, and sport of this kind is, perhaps, an expression of the general play instinct. "Intelligence," as Lindley holds, "is no exception to the law of exercise. Just as those animals, which by fortunate variation were born with a tendency to indulge in preliminary exercise of those activities which were to serve the serious ends of adult life, were favored by natural selection, and were able to transmit such advantages in the form of general play instincts, so in a more special way those creatures, endowed with the strongest tendencies to exploit the intelligence, may have perpetuated this superiority as a general intellectual play instinct."[1] Again, the chess strategy of an individual is largely the product of his own brain; it is his own, and merely as such is interesting to him. No matter where or how he got his knowledge of the game, if he is anything of a player, he has assimilated it and made it a part of his mental self, and his game, in turn, reflects something of his personality. There is also what might be termed a secondary, derived or æsthetic interest in chess, namely, in the finer and subtler points of the game, in what the chess world calls its "brilliancies." Appreciation of and consequent admiration for the skill of others is a contributory element in this pleasure. And finally it is, notwithstanding its own exacting demands, a means of mental relaxation and as such is attractive to the mental worker.

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[1] Lindley: A Study of Puzzles, *Am. Jour. Psy.*, VIII, 1897, pp. 431-493, especially p. 437.

[2] Groos: *op. cit.*, pp. 369 ff.

[3] Groos: *op. cit.*, p. 385.

[4] In order to supplement my own observations and those of my assistants, a questionnaire was submitted to chess players of different grades of ability. The list of players answering is a fairly representative one and contains the names of some of the best amateurs of the United States and Canada. Some of the data from this source are specifically included in this study, but in many other cases the substance of the views expressed has been incorporated without more acknowledgment than is here made. About 100 answers in all were obtained.

2. General Features of Chess from a Psychological Point of View.

The Emotional Effects of Play. We have already alluded incidentally to the emotions which may be stirred by the chess combat. The desire to win is fundamentally connected with the fighting instinct.[2] Young and ardent players especially find the elation of victory and the bitterness of defeat by no means small; they work hard at the game and feel the outcome in proportion to their efforts. The chess manuals and magazines repeat suggestions as to how one should wear his laurels or accept defeat, but in spite of this well intended advice every chess club has its members who invariably make excuses for every lost game. A good many players, however, have the sportsman's feeling strongly developed and are not unpleasantly affected if they are conscious of having played well. They do not enjoy winning if their victory is the result of a "fluke" on their own part or of a palpable oversight on the part of their opponent.

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[1] Lindley: *op. cit.*, p. 437.

[2] This instinct in man, we are told, is being gradually overcome or suppressed. It would be interesting to note, however, whether in the contests which still give opportunity for it, there is any lessening of the desire to win, and whether individuals change at all in this regard. The fact probably is that the instinct is changing its form with social pressure, but losing little of its native power.

Personal and Temperamental Differences of Players. The opinion is general among chess players that a man's temperament enters into his play and determines its style. Many of my correspondents state that they have recognized and often utilized this factor in actual play by forcing an opponent to adopt a line of play for which he was unfitted by temperament. For example, a slow, careful game is played against the aggressive and daring player, who is often provoked by these Fabian tactics into recklessness and loss.

Chess players are also very firm in the belief that one's game is an index of his character in a wider sense, and no one will be likely to deny that the fundamental traits of character are revealed in unimportant matters especially when one becomes so deeply absorbed that he forgets all else. Chess offers just such an opportunity for deep absorption, and it is not unreasonable to suppose that one's real rather than his conventional character will reveal itself.

3. Attainments of Players of Average Experience.

In order to form some conception of the skill and knowledge which a chess player of average experience possesses, let us consider (a) his ability to plan moves ahead and to anticipate those of his opponent; (b) to disentangle a complicated situation; (c) to reconstruct the status of an unfinished game from memory; and lastly, (d) his "position sense." For information on these points I shall, of course, have to depend almost wholly on the replies of my correspondents.

Ability to Plan Moves Ahead. It is evident from the variety of answers to the questions on these points and the qualifications attached to many of them that the questions were interpreted in a variety of ways. Some points seem clear, however. The number varies from position to position, is dependent upon the number and positions of the pieces and the player's physical and mental condition at the time. Very few stated any definite number of moves which they thought they usually planned ahead, but allowed a considerable margin. The following are typical of the answers received:—"five to ten," "two to six," "two to ten," "six to ten," "three to seven."

Very few state that they are unable to plan at least three moves ahead in a complicated situation. Four, five, and six are favorite numbers.

Most state that they can anticipate as many of their opponent's moves as they plan for themselves, and that they do so habitually. A few state, on the other hand, that they can anticipate only a much smaller number of their opponent's moves.

Almost without exception my correspondents write that practice has greatly increased both numbers, but especially the number of the opponent's moves that can be foreseen. A few who have played a great many years or who seldom play now, say that the number has decreased. While with most players the increase in number has been considerable, the increase in accuracy has been the main gain. The beginner, owing to the great number of possibilities, is not able to plan far ahead and scarcely thinks of his opponent's plans at all. A little later he plans two, three, and four moves, but he overlooks so many possibilities that his plans are practically worthless. Progress in this regard consists first in the increasing ability to perceive the most likely and feasible continuations both on his own part and that of his opponent; second, in refusing to reconsider lines of play after going over them carefully once and discarding them; and third in increased ease, rapidity and accuracy in calculation.

Visual Imagination. It was asked "Can you imagine, pictorially, what difference in the position a move would make, or are you absolutely without such an image, relying wholly on successive associations of one move with another?" The answers seem to indicate that there are three classes of players in this regard. There are, first, those who have a clear visual 1
picture of the situation as it will appear after a series of moves; secondly, those who have some visual picture, but rely also on 2
successive associations, in verbal or possibly motor terms, of one move with another, that is, they are unable to picture a resulting situation, but must build it up move by move by means of visual and other kinds of imagery. With these players the final term is probably held in verbal terms. The last class 3
of players are those who are without a visual image of any sort. The first class is perhaps the smallest. The players in this group state that the presence of the pieces is not only not an aid in planning combinations, but that it is a positive hindrance. They have difficulty in imagining a piece in a changed situation or on a square which is at that time occupied by another piece when the pieces are on the board before them.[1]

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[1] Binet: *op. cit.*, p. 236.

Binet quotes Selkirk approvingly as saying that in working out a plan one is obliged to represent to himself the position of the pieces after each supposed move and that the sight of the board only confuses. Dr. Tarrasch, the German master, holds that all games are played in part without sight of the board and that consequently visual imagery is an essential factor in planning moves ahead, especially in far reaching combinations.[1] This statement, it seems to the writer, is valid only for players of the first class mentioned above. The players of the second group have some picture but find the presence of the pieces indispensable; while those of the third group rely wholly on the presence of the pieces. In some cases this dependence on the pieces is largely a matter of habit, since the players state that while they rely almost exclusively on successive associations, nevertheless, they can often discover errors in their games when the board is not before them.

Ability to take in Large Sections of the Board. Most of the players state that when getting ready to move they can readily take in the whole disposition of their men, or, in other words, they can comprehend the board as a whole. This ability to take in readily the whole disposition of the men is generally regarded as one of the signs of a considerable degree of chess skill. Ability in this regard varies with the physical and mental condition of the player and with the complexity of the situation. The explanation of the gain in skill of this sort seems to be that, as a player progresses in skill, the game takes on more and more meaning and that the individual moves become more and more a part of a definite series or of a number of series each with some particular end in view. The different moves and situations, also, as they are handled in larger masses, are dealt with in an increasingly symbolic manner. A more detailed consideration of this will be taken up in another section.

Reconstruction of the Status of an Unfinished Game. Little or no trouble is experienced by most players in setting up an unfinished game from memory, provided the game itself was interesting and too great a time had not elapsed. The number of pieces on the board is also a factor, though it would appear that it is not of very great importance. A very few state that they can do this only when they are playing regularly. One player reports that he retains a position in correspondence play for a month without difficulty, and another that he is engaged at present in eleven correspondence games and that he retains the positions in all of them without reference to his record.

Different methods are employed in the reconstruction, but all are reducible to two types, namely, setting up the final position, and replaying the game from the start. Some are able to do either. There are different varieties of the first method. Some seem to have a mental picture of the whole board and to arrange their pieces accordingly. They have photographed the situation as a whole and the eye tells them if anything is out of order or missing. Analogous to this in a small way is the ability to see a misspelled word in proof-reading. Others also reconstruct the final position as a whole, but do it by remembering crucial situations and building around them. This memory may be in terms of almost any sort of imagery, but it is most likely to be in visual terms. Verbal imagery also plays an important part. The plan of attack or a certain situation in that attack, is very often the central point from which the position is built up. This would mean that the steps which had been planned ahead were also factors in the recall. Sometimes it is necessary to begin back of the final position at some important place and to build up to it. The second method, that of replaying the game from the beginning, means the running over of a series of successive associations aided and guided by the critical points and by the general plan of the whole game which gives a meaning to the individual moves. The reconstruction from memory of a position involving any considerable number of pieces is not possible to most beginners. If they are of the photographic mental type they get lost in the mass of impressions which the situation involves, and if of the verbal or some other type the situation has not sufficient meaning to give definite place and order to so many pieces.

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[1] Binet: *ibid.*

"*Position Sense.*" Among chess players and writers on chess great stress is laid on what is called "position sense," that is, the knack of knowing in an intricate situation how to place the men to the best advantage. It is a common observation that many chess players are able to tell at a glance which player has the better position without being able to give offhand any reason for the opinion. It is even stated that many players are able to give a correct judgment at times without being able to carry out the analysis necessary to prove its correctness. Bird, the well known English player, used often, in consultation play, to point out the move with the remark that the others might analyze as much as they liked, but that he felt and knew that it was the right move, and it is said that he was generally right. With scarcely an exception all who answered the question stated that they have noted a considerable improvement in "position sense." Many state that improvement in the sense of position and chess improvement are one and the same thing. This latter statement is a little too sweeping, however, since it does not necessarily follow that the mere knowledge of the strength or weakness of a position will enable one to choose the best of the infinite possibilities which arise at every step. Experience is the blanket term which most use in the attempt to explain the development of "position sense." The player is said to "feel" the position or the proper move. Some interesting reasons are given, however, to account for the ability to judge a position at a glance. In brief they are somewhat as follows: The mind has been drilled to feel any deviation from principles; it is due to a vague idea of similar situations leading to success or failure; it is the recognition of several fundamental points of strength or weakness; and lastly, it is a symbolic shortening, a dropping out of intermediate processes of inference. Perhaps we should not be wrong in saying that it is all of these. It is undoubtedly the product of experience and involves the same sorts of psychic processes that are employed in the formation of general ideas—abstraction and generalization. Players of equal experience differ so widely in "position sense" that it seems reasonable to suppose that there is a difference in their native endowment in this capacity, just as, according to Professor James, people are differently endowed with the capacity for memorizing. "Position sense" is, however, not dependent on memory alone.

Different Grades of Chess Players. Certain mental qualities are essential to the chess player who attains any degree of proficiency whatever, and players differ both in their relative and their absolute endowment of these qualities. Master players combine to a marked degree an accurate and persistent chess memory, quickness of perception, strong constructive imagination, power of accurate analysis and a far seeing power of combination. It is impossible to say just what the proper proportion of these qualities should be, and to be ideal it would have to be modified to meet each new opponent. When these various qualities are combined in something like the proper proportion we have what is generally designated as a separate quality, namely, "judgment." But when we say that a player has good judgment in chess do we mean more than that he combines in something like the proper proportions the qualities which make up the uniformly consistent and successful chess player?

4. ATTAINMENTS OF THE CHESS MASTERS.

We have attempted to give some idea of the endowment of the chess player of fair ability and have avoided all reference to the remarkable achievements of the chess masters. The feats of some of these are certainly marvellous, and one is apt to think that genius alone can account for them. The chess expert displays his skill under one or all of four forms, namely, Simultaneous Play, in which several games are played at the same time against as many opponents; Blindfold Play; Recapitulation of Games played by himself or others; and, in actual play, by the Announcement of the End of the Game several moves before that event.

Simultaneous Play. In simultaneous play the lone player, of course, never plays against those of his own rank, but usually against strong local players who are able to take advantage of any oversight. As examples of what can be done, the following, recorded in the different chess magazines, may be cited:—Gunsberg played eighteen games simultaneously against as many opponents, winning fourteen, losing three and drawing one; Bird played nineteen, winning fifteen, losing one and drawing one; Herr Schallop in four and one-half hours played forty simultaneous games, winning thirty-three, losing five, and drawing two; Lasker played twenty-two games, winning nineteen, losing two and drawing one. Since that time he has often played thirty games simultaneously. As an example of the rapidity of moves made in simultaneous play Napier's twenty-one games should be cited. During the first hour he made four hundred and fifty moves, an average of nearly eight per minute. Of the twenty-one games he won eighteen, lost two and drew one. Evidently simultaneous play requires the ability to focus the attention strongly on a single game, to banish for the time being every other game from the mind, to call up instantly at the sight of any board just what combinations it had been planned to carry out there, and finally to recognize and meet a situation promptly. In all such feats experience is an indispensable factor. The player who plays several games at the same time relies on his knowledge of position, gained through long practice, to give him a quick grasp of the essential situations as he passes from board to board.[1] This factor and the power of concentration seem to account for the distinctive features of simultaneous play.

Recapitulation of Games and Other Feats of Pure Memory. The recapitulation of games is a feat of memory pure and simple. The player simply plays over, or dictates from memory games which he himself or which others have played. The games thus enumerated may consist of fifty or even more moves on each side making sometimes a total of one hundred or more individual moves. Morphy, the next morning after his blindfold contest against eight other players at Paris, dictated to his secretary all of the moves in each of the eight games. Morphy's secretary, in his book entitled "Exploits and Triumphs in Europe of Paul Morphy," gives the following account of the performance:—"Next morning Morphy actually awakened me at seven o'clock and told me if I would get up he would dictate to me the moves of yesterday's games. I never saw him in better spirits nor less fatigued than on that occasion, as he showed me for two long hours the hundreds of variations depending upon the play of the previous day, with such rapidity that I found it hard work to follow the thread of his combinations."[1] In speaking of Morphy's knowledge of games played by Anderson, he writes: "With his astonishing memory he gave me battle after battle with different adversaries, variations and all."[2] And in another place, "What wonderment he has caused with his omnipotent memory. I have seen him sit for hours at the Divan or the Regents, playing over, not merely his own battles, but the contests of others, till the spectators could not believe their senses."[3] Morphy himself made the statement that he had never forgotten a game that he had played after his chess powers were mature. Blackburne likewise has a tenacious memory for his past games. In 1899 he recalled any number of games which he had played in 1862, pointing out with the utmost precision the flaw or the beauty in each.[4]

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[1] A player of perfect "position sense" could play any number of games *ad hoc* without recalling anything.

In regard to the recapitulation of games it should be noted that the player is recalling a number of known situations each the result of a well known series of moves, and that each game as a whole is constituted of, and characterized by, a number of situations joined together by distinctive features which may consist either of individual moves or of combinations of them. The case is similar to that of a remembered conversation; the one who recalls it does not recall each word separately but rather the *meaning* of each remark and its connection with what preceded or followed.

Other Feats of Memory. Blackburne, without sight of the board, is able to give the moves known as the knight's tour, which consists in placing the knight on any designated square and making it strike in succession every square on the board. This is by no means an easy task with the board in sight, a fact of which any one may easily convince himself. Aside from chess Pillsbury performed some rather difficult memory feats. If any portion of a deck of playing cards was called off to him, he was able to name the cards remaining in the deck. On one occasion, after playing blindfold games for two and one half hours, during the intermission, a list of thirty words, numbered from one to thirty, was read to him. He memorized the words in groups of five taking ten minutes in which to complete the task. Then he was able to give the word corresponding to any given number or the number corresponding to any given word and to repeat the whole list either forward or backward.[1] He made use, of course, of some mnemonic device and the case is interesting only as showing what can be accomplished in that way.

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[1] Edge, F. M.: Exploits and Triumphs in Europe of Paul Morphy, N. Y., 1859. p. 164.

[2] Edge: *op. cit.*, p. 187.

[3] Edge: *op. cit.*, p. 187.

[4] Graham, P. Anderson: Mr. Blackburne's Games at Chess. London, 1899. p. 207.

Announcement of Mate in Advance. The announcement of mate several moves ahead means, in case it is not merely a remembered position, that the player has looked ahead of the actual play and is able to say the precise number of moves necessary to bring about the end of the game. It is a common thing for players of the first rank to announce mate five or six moves in advance and their combinations in the middle game often reach beyond that number. Blackburne, in one of his blindfold performances, after the twentieth move, announced mate in six moves more and then called off seven variations which exhausted the position.[2]

Marshall, in London, announced mate in eight moves and proceeded to accomplish it in spite of all his opponent could do to prevent it. The longest mate ever announced in blindfold play was one by Blackburne in sixteen moves.[3]

In some cases planning ahead, as was suggested above, is a simple act of memory. The player may merely recognize the situation as one previously seen and may remember the individual moves which followed and the result; or he may pass directly from the first term, the situation, to the last term, the result, recalling at the same time the number of moves, but not the moves themselves. Where the player has never reached mate from the given situation, but is able to foresee it, he must possess the ability to work through mentally all the situations which come between the one given and the final one, which calls for good powers of analysis and memory as well as experience.

Blindfold Play. The feats which have caused most wonder and admiration are those of blindfold players. Playing without sight of the board is now one of the most common forms of exhibition chess, and it has been said that almost every good amateur can play at least one game *sans voir*.

Paul Morphy, during his triumphal tour of Europe, created great astonishment by playing eight simultaneous games blindfolded. It is said by competent judges that some of his most brilliant games were those played in this way. Zukertort played twelve and fourteen games very frequently, but often remarked that the two additional ones made it much more difficult. Blackburne is one of the strongest of the blindfold players, but the greatest of all thus far in this line was the American, Pillsbury, who played as many as twenty-two blindfold games simultaneously, winning most of them. With him the number of games seemed to be limited only by the length of time required to complete them and by his physical endurance.[1]

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[1] *British Chess Magazine*, Vol. XX, p. 399.

[2] Graham: *op. cit.*, p. 209.

[3] Graham: *op. cit.*, p. 211.

Pillsbury, in an exhibition given at Toledo, Ohio, played twelve games of chess and four of checkers without sight of the boards, and at the same time played duplicate whist.

Such are the feats of blindfold play by the masters. What shall we say in explanation of them? Memory certainly plays a very important rôle, but it may be chiefly of the short time variety, that is, the player holds the moves in mind only during the progress of the play and forgets them immediately afterward, much as the student or the lawyer does the facts he has crammed for a particular occasion. Pillsbury, in an article, said that he had to think rather hard to recall the opening in any given game of a series five minutes after the contest ended.[2] Morphy, on the other hand, seemed to retain his games permanently.

A blindfold player, playing a single game, must have in mind at every stage of the game the position of every piece on the board, and he must have some way of knowing or of calculating the relation of each piece to every other, facts which are not necessarily involved in mere place memory. His knowledge of these positions and relations must be sufficiently clear to enable him to form combinations for attack and defense. In playing eight, ten, twelve, sixteen or twenty games simultaneously without sight of the boards, the task is, of course, immensely more difficult, since the player has not only to remember a proportionally greater number of moves (or positions) but has also to remember each move or set of moves in relation to the particular game in which it is made.

The blindfold player, playing several games simultaneously, usually employes devices to make his task less difficult. Pillsbury grouped his games and used the same opening in all games of a group. For instance, in playing sixteen games he grouped them as follows: group one contained boards 1, 5, 9, 13; group two, boards 2, 6, 10, 14; group three, boards 3, 7, 11, 15; and group four, boards 4, 8, 12, 16. It will be noticed that two groups contain odd and two even numbers, and that there is a difference of four between any number of a group and the one next to it—1, 5, 9, etc. The blindfold player usually has first move on all boards and can generally force his opponent into his system. If not, he may regroup the boards according as they do or do not fall into his system of play, or he may simply make a mental note of the boards on which eccentric replies to his opening moves have been made. Obviously, so long as the games in each group proceed without variation from the usual moves and replies, there is little chance for confusion, but very soon the game begins to vary. By the time this happens, however, the player has noted some distinctive feature by means of which to recall any game. Pillsbury put it in this way: "By the time twenty moves have been made there has been some clearing of the board and a definite objective has been developed. When I turn to the new board I say—Ah! number nine. This is the board on which we have exchanged queens; and the whole play comes back to me."[1]

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[1] The physical endurance required for play in such contests is something little realized by the uninitiated. Morphy, at Paris, played for ten consecutive hours without eating or drinking anything. Paulsen, who played as many as ten games blindfold, played twelve consecutive hours on one occasion with no refreshments of any kind except a little lemonade.

[2] Pillsbury: The Chess Player's Mind. *Independent*, Vol. LII, p. 1104.

In other words, the variation itself, because it involves some distinctive feature, is the cue for the recall of all the moves that have preceded it and those which grew out of it. It will help us to understand this if we recall the fact that chess is, as Binet puts it, a contest between ideas, and that each move is but a part of a series all working together toward the same end, or in other words, each move is remembered because it is a necessary part of a plan.[2]

He points out further that those who retain in mind a situation or a series of moves have the faculty of giving to the situation or the series a precise significance. A person ignorant of chess could not, of course, do this and so would be unable to hold such things in memory. Mr. R. L. Newman also, experimenting upon checker players in the laboratory of the University of Indiana, found that a long series of moves in checkers, made in the presence of his subjects, was remembered only after some form of grouping was employed and that the series was learned quickest by those who understood the purpose of the different moves.[3] The purpose caused the individual moves to hang together, so to speak.

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[1] Pillsbury: *op. cit.*

[2] Binet: *op. cit.*, p. 264, p. 274.

[3] Mr. Newman's article has not been published, but the manuscript was placed at the writer's disposal through the kindness of Prof. E. H. Lindley of the University of Indiana.

Binet[1] concludes with M. Goetz that the memory employed in games without sight is above all a memory of reason and calculation, or more concretely, that one does not remember that he has moved his king at such and such a time, but remembers a certain project of offence or defense in accordance with which he has moved his king. He qualifies this in part, however, by the statement that sometimes individual moves which make a deep impression on the mind and awaken astonishment are recalled individually. One retains a game of chess as he does a printed line or paragraph; the meaning and not the individual letters or words are what is retained.

Both Taine and Binet have studied the question of the visual representation of the board by the blindfold player. Taine concluded that such a player sees the board and the pieces on it as in an "interior mirror." He quotes an unnamed American to the effect that at the beginning of a game he sees clearly before his mental eye, the board and the exact appearance of each piece, and that after the announcement of each move he sees the pieces in the new arrangement, in exactly the same way.[2] The method of Dr. Tarrasch is thus described by Binet:[3] At the start he represents the board in its original condition. When he makes the first move he sees the board thus modified and keeps the new impression in his mind's eye, and so on through the game, his mental picture changing with each move. Binet's correspondents, with one exception, answered that they used visual memory in playing without sight. He concludes from their answers that there are two forms of visual memory used in blindfold chess, which he designates as concrete visual memory and abstract visual memory. Players who make use of the former see the forms and the colors of the pieces and squares on the board exactly as they are. Abstract visual memory is described as follows: Most of the players see the board mentally. The mental image is localized before the player, but he apperceives at one time only the part of the board where the interesting features of the battle are taking place. The board does not ordinarily have a particular form. It is an abstract board composed of sixty-four squares. Very often the edges of the board are not seen. For some players certain diagonals, having particular importance for the game, are seen more clearly than others. Often the colors of the squares are not clearly seen, but become grayish, one color being a little brighter than the other. The form seems to be the element which is the most difficult to efface from the mental image.

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[1] Binet: *op. cit.*, pp. 270 ff.
[2] Taine: On Intelligence. New York, 1899. pp, 38, 39.
[3] Binet: *op. cit.*, pp. 276 ff.

What Binet calls the geometrical notion often takes the place of color. Binet's correspondents are unanimous in the opinion that they represent to themselves the positions of the pieces and their spatial relations and that no combination would be possible without such representation. Charcot gives the name "geometrical visual memory" to that kind of visual memory which simply conserves the positions and the movements of the pieces.[1]

In agreement with Binet we may say that this kind of memory is the work of abstraction and results from the direction which the player gives to his attention. Form and color are neglected because they are of little importance. This abstraction, as Binet points out, is comparable to that in daily life where we gradually eliminate details and give attention only to essentials.

It seems evident from Binet's study, and from the statements of many chess players, that visual imagery in varying degrees of clearness from the most perfect representation to the most shadowy, is a very important factor in playing chess without sight, and that most players make use of it; but there is, on the other hand, data to warrant us in saying that it is not an absolutely indispensable factor. In other words, it is possible that a blindfold game could be carried on by a person entirely devoid of visual imagery. M. Goetz, in his paper published in Binet's book, says that visualization is almost entirely absent in his blindfold play and that his performance depends only on "reason and calculation."[2] For example, he knows from experience that a pawn on the king's fourth attacks one on his opponent's queen's fourth, and that a knight or a bishop on a certain square controls certain other squares; and this knowledge may be retained in verbal terms. Pillsbury, the greatest of all blindfold players, also asserted that he had little or no visual imagery and that he remembers each board and the positions on it not as a picture, but as a record.

Even in my own limited experience in blindfold play, I have found that visualization is an incidental and by no means essential factor. In my own case, in the beginning, visual images were entirely lacking, a little later they were present at times as the result of a conscious effort to call them up, and now when they are present they are so only in the most indefinite form. For instance, I have no mental picture of the board aside from its general outline, and the forms and colors of the pieces are never present, except when I have paid particular attention to them for experimental purposes. In the beginning, localization of the play was very indefinite and a replaying of the games with the board furnished many surprises both in this regard and in regard to the relative positions and distances of the pieces. At the present time the movements of the pieces and the localization of the play are fairly definite. I seem to feel the movements of the pieces, especially my own, as if I were actually moving them. Particular positions involving two or three pieces are sometimes seen in so far as the relative positions of the pieces are concerned. Normally I am a fair visualizer, but in blindfold chess my thinking seems to be largely of other sorts, and especially in verbal terms. When not engaged in actual play I frequently call up a situation with a fair degree of clearness, but when playing, verbal imagery is the most prominent in consciousness. For example, my opponent announces knight to the king's fifth. Ordinarily I do not picture the resulting position, but calculate the radius of action of the piece thus: knight on king's fifth attacks queen's seventh, bishop's seventh, etc. If it is advanced to queen's seventh it checks king at knight's first, etc., etc. It would seem that there is a closer association between the series of verbal images than between the visual images or the series composed of both verbal and visual images. My experimenting has not gone far enough, however, to furnish very much that is definite in regard to this aspect of the question. Without visual imagery the blindfold player would have to rely on word, letter and number symbols, and would have, it would seem, a much more difficult task than the player with highly developed power of visualization. In actual play, verbal memory plays an important part even for strong visualizers, for it is often by this means that they recall the actual moves that have been made when they are in doubt as to the position of any piece. My companion in the attempts at blindfold play made considerable use of visual imagery of Binet's abstract type, but used other sorts to a certain extent. I am inclined to believe that with increasing experience both of us would have made more use of verbal and other symbols.[1]

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[1] Binet: *op. cit.*, p. 311.

[2] Binet: *op. cit.*, pp. 340-351.

In order to determine whether it would be possible to play chess with no visual imagery whatever, the following experiment was tried. Games were played without the use of either board or chessmen. The records were kept in the German notation, but in such a way that each player could tell the number and the location of the pieces on either side. The moves and replies were thought out as far as possible with the aid of this record and in terms of the symbols used. For instance, P a 5 attacks any pawn or piece on b 6; Kt c 3 attacks b 1, a 2, a 4, b 5, d 5, e 4, e 2, d 1, etc. It was thus possible to calculate the relative positions of the different pieces and to attack and to defend any given position. The experiment was not long continued and visual imagery was never wholly absent, especially where attempts were made to form combinations. Nevertheless, I am convinced that it would be possible for a person to learn to play chess by means of verbal and number symbols alone. The task would be a very long and difficult one, but by no means impossible.

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[1] It may be conjectured that the necessary concentration of attention on the *relations* of the pieces rather than on the pieces themselves is partly responsible for the incomplete development of visual imagery.

The Relation of Chess Skill to General Mental Ability. If chess is perhaps a tolerable index of temperament and character, is skill in chess also a reliable index of mental power in general? The reply must be qualified. Many able men are good chess players, but on the other hand there are those who live for chess, who think, talk, and dream chess, who confirm Edgar Allen Poe's observation that the best chess player may be only the best player at chess; but this number is small compared to the vast majority who indulge in it only as a pastime. Even among chess masters are to be found many who have displayed considerable ability in other lines. Dr. Emanuel Lasker, the present world's champion at chess, has taken his doctorate in mathematics. Tschigorin is a Russian government employee, Maroczy is a professor of physics and mathematics at a Budapest college, Tarrasch is a German physician, Anderson, at one time champion of the world, was a professor of mathematics, and Staunton, another world's champion and one of the best known of the older chess writers, is well known also as a writer and as an editor of the classics. Rousseau, Voltaire, Napoleon, and John Stuart Mill are said to have been strong players, and the historian Buckle an excellent one. The list might be increased almost indefinitely, but enough has been said to indicate that skillful chess players represent all walks of life, and that skill at chess is not incompatible with success in other lines. The chess player is usually something more than a mere player of chess. At the same time the cases of *idiots savants* in various forms of mental activity, and among others in chess playing, prevent the inference that skill in chess is a universally valid index of high mental endowment.[1]

5. SPECIAL PSYCHOLOGY OF CHESS.

Forms of Mental Activity Required. We have now followed sufficiently, perhaps, the general aspects of the game, and can turn with advantage to its more intimate psychology. The aim of each player is, as we have said, to checkmate his opponent, that is, to bring his own pieces into such a position that the opposing king could inevitably be taken at the next move. Each player must therefore carry out a scheme of attack, overcoming obstacles and preventing the blocking of his own plans, and at the same time guard himself from counter attack. The game in its most important portion presents in essence a succession of situations each of which calls for special examination, with reference both to its present and its future import, and the selection or invention of an appropriate line of action. The player asks himself continually, in effect, at least, what is this present situation and what ought I to do to meet it? The game throughout may be regarded as a series of reasoned inferences, expressed by moves upon the board. The present section will be devoted to an exposition of the logical and psychological relations in question.

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[1] See in the appendix to this study an account of a fair chess player of otherwise feeble intelligence.

The Stages of the Game and Their Logical Types. The game of chess proper is divided into three fairly well-defined parts called the opening, middle, and end games. There are openings without number but all have been the subject of analysis for so long that one can obtain from the numerous books on the subject information limited only by his capacity to retain it. The competent player knows at least the chief openings and enough of their theory to meet any unexpected variation from the usual moves and replies.

The end game, in which the forces on either side are greatly reduced, has also received careful study at the hands of expert analysts, so that one may learn from the books to recognize certain situations and to know their possibilities. Geometrical figures have often been employed to show the possibilities of situations.

In the middle game, however, the player may no longer rely on definite directions, but is entirely dependent on his knowledge of general principles and his past experience. The former will be of service especially to the young player, but, owing to the infinite number of possibilities which may develop out of the different situations, experience in actual play is indispensable. Here the player must exercise all his ingenuity, must give rein to his creative imagination, and must follow out as far as he is able the effects of the different moves which suggest themselves. The chess player's skill is measured in terms of his ability to do all this successfully.

Opening and End Games. In the opening game and in the end game the logical type of reasoning is usually that of the categorical syllogism. In case of a typical opening it may be formulated as follows:

In all cases of the Evan's Gambit, pawn to the queen's knight's fourth is the fourth move.

This move is to be the fourth in an Evan's Gambit.

Therefore, this move should be pawn to the queen's knight's fourth.

Similarly in the end game the situation which develops recalls the procedure to be followed. If White, for example, has a king and a rook against Black's king, he must drive the latter to the edge of the board, hold him there with his king and mate with his rook. White's procedure may again be reduced to the type of the categorical syllogism.

All cases of king and rook against king are to be met by driving the latter to the edge of the board, etc.

This is a case of king and rook against king.

Therefore, this is a case to be met by driving the king to the edge of the board, etc.

All habitual actions may be reduced to this type and Professor Charles Pierce has remarked the same about all reflex actions.

The Middle Game. In the middle game, where general rules are only partially applicable, the logical procedure is mixed and will differ somewhat according to the grade of the player. In what follows immediately we shall assume the player's condition to be that of a not very skillful amateur; of the professional's condition we shall speak later. So far as general rules apply to the middle game, the play will be of the deductive type which we have just illustrated, but in the vast majority of cases it will be more complicated. The situation is not of the known sort that invites application of general rules, but of an unknown sort in which the essential features (or true meaning) must be disentangled from a mass of obscuring details, and when disentangled must be met by a move or a line of play especially selected, or invented, for the purpose. The logical type is not now simply deductive, but really a series of logical steps resembling the sort of scientific procedure which Jevons, for example, calls the "Combined or Complex Method."[1] An hypothesis is first formed, deductive inferences drawn from it, and these tested by experiment. The player finds before him a situation created by the last move of his opponent. His study of the situation gives it a certain character in his mind equivalent to the formation of an hypothesis with regard to it. He then reasons: This is a situation of such and such a sort and therefore to be met by such a move in reply. The move in reply is then tried in imagination. If it seems successful it is accepted and actually made; if it is seen to be unsatisfactory, it is rejected and a better one sought for the same purpose, or what is more likely, the hypothesis itself (the conceived character of the situation) has been changed by the evident unfitness of the move imagined.

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[1] Jevons: Lessons in Logic. New Ed. London and New York, 1905. p. 258.

Skill is shown in the opening and end games by the readiness with which the player recognizes the common situation and draws from memory the appropriate response. Skill in the middle game is shown by the readiness with which he recognizes the essential features of a new situation, and, in his inner experimentation, hits upon a move that fits the case, *i. e.*, proves by its appropriateness that his diagnosis of the situation was correct.

This is the condition of the commonplace player. The case of a perfect player, one with chess omniscience, whose analysis was perfect, who could see the game to the end at any stage of it, would be quite different. Having a perfect plan of procedure for every case, he would play throughout very much as the amateur plays the opening and the end games. Excellence in play ranges upward from the condition of the amateur toward that of the perfect player. To the chess master many of the situations that arise in the middle game are already familiar and the best means of meeting them known. Others will be unknown; and then the crucial point of his opponent's attack must be discovered and an appropriate response devised. His play is for the moment of essentially the same type as that of the amateur, except that he is both by nature and experience much more prompt in discovering the essential feature of the attack and much more resourceful in finding means of repelling it.

Let us, however, return to the logical type employed by the commonplace player. The type followed in the opening and end games would correspond closely to the typical logical procedure as described by James.[1]

The type followed in the middle game differs from the formal sketch of James which has in view reasoning of the deductive type. Here the essential characteristic of the situation, even when discovered, does not suggest any well known group of similar cases to which it may be referred and for which a definite mode of procedure has already been worked out. The essential characteristic can at the most suggest only a very general kind of procedure; it gives no inkling of just what should be done. The player knows that he must sacrifice the threatened piece, or withdraw it, or intercept the attack, or make a counter attack, but which of these is best must be thought out for each situation. His usual method is to try in imagination one move after another until he finds one that seems superior to all the rest. And often it is only during this experimental process that the full signification of the situation dawns upon him.

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[1] James: Principles of Psychology, N. Y., 1899, Vol. II, pp. 330 ff.

Such reasoning is concrete and practical, not put into words, or only partially so, and allied to the reasoning of animals and children.[1]

But, as Morgan well shows, the logical reasoning of man is largely dependent on the need of communication and the use of language;[2] a chess game *played* is reasoned in particulars; the same game *explained* and *defended* to a companion would be cast in verbal and syllogistic form.

Psychological Restatement of the Logical Types. This last remark touches upon an essential point to which we must give yet a little further attention, namely, the difference between the logical types of reasoning and the actual psychological processes which they symbolize. All processes of reasoning are, as psychological facts, sequences of mental states due to shifting of the focal point of attention and to processes of association dependent thereon. In the deductive portions of the game—the opening or end game, where the play is guided wholly by rule—the process is one of serial association running off under the general influence of the conception of the opening (or end game) which remains in the background of consciousness. Each move suggests the next in fixed sequence, as one might say the alphabet, having in the background of his consciousness the desire to say it.

For the middle game let us take a concrete example. Let us say that it is Black's turn to move. He glances at the board and notices the queen and knight of his opponent in position to develop a double check upon his king. Association, under the guidance of his general knowledge of the purpose of the game, freely suggests the consequences, if he cannot in some way interfere. Attention then shifts to the response to be made and association again coming in suggests the readiest means of defense. In other words, the situation, regarded from the point of view of defense and held in the focus of attention, recalls by association a number of possible moves. These associations, following, of course, the readiest lines of habit, are not by any means at random, but operate strictly within the limits imposed by Black's knowledge of the general rules of play and his present intention. Each of the moves suggested is itself brought to the focus of attention, is tried in imagination, probably by incipient movements of eye or hand,

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[1] It is what Romanes calls reasoning in particulars. Romanes: Mental Evolution in Animals. N. Y., 1900. p. 337.

[2] Morgan: Introduction to Comparative Psychology. London and N. Y., 1902. pp. 293 ff.

and accepted or rejected as the case may be. If accepted, it is put into execution in the same manner as other voluntary movements.

The mental action of the player in such a situation is analogous to that of the inventor. A half finished machine stands before him; his problem is clear; he must cause such and such movement in such and such parts in order to bring about a desired result. He runs over in his mind the varieties of pulleys, cranks, gearings, cams and the like with which he is familiar, and finally selects one or the other as the most likely to accomplish what he wishes. A high grade of skill as an inventor or as a player of chess involves the utmost readiness in seeing just what needs to be done and in discovering the means of doing it. Experience helps immensely in both of these directions; and it brings many cases under fixed rules so that they are dealt with by simple associations and correspondingly reduces the number of cases that must be treated as singular and without rule, and greatly enriches the fund of expedients that may be tried in such singular cases. When the case is so unfamiliar that experience suggests nothing, the reasoner is reduced to simple blind fumbling, on a level with that of the brutes, and rational procedure reduces to the "method of trial and error." The situation arouses an impulse to do something; there is a blundering attack; efforts that lead to unpleasant consequences are rejected; those with pleasant consequences are repeated. Man's more complex mechanism of apperception, his wider range of associations, and his power of imaginative action all combine to reduce the cases where blind fumbling is necessary, but when these powers are of no avail there is but one method, and that is the method of lucky hits.

II. THE PSYCHOLOGY OF THE LEARNING OF THE GAME.

1. *General Description of the Learning Processes in Chess.*[1]

In the preceding sections have been set forth what I conceive to be the general outlines of the mental activities involved in chess playing. It is popularly believed that chess is a very hard game to learn, that it is difficult for every one and impossible for many. To a certain extent this is true. Chess is a difficult game, but it is so because it requires a peculiar mental equipment, rather than because it calls for one of an especially high order. First and foremost is required a liking for chess. The man who finds it uninteresting may as well give it up at once. Next it requires powers of sustained attention and an excellent memory;[1] and based on these, considerable powers of analysis, and visual imagination, or its equivalent in some other sense department.

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[1] My sources of information here are my own introspective notes while learning to play, and those of four assistants, together with the replies of my correspondents.

Increase in skill means increase in the knowledge of chess situations and how to meet them; or, in more psychological terms, increasing "meaning" in certain arrangements of the pieces,[2] and increased facility of association between these meaningful arrangements and certain other arrangements (moves to be made) imaginatively constructed; or, in still other terms, more adequate apperception of the situations and richer and better organized associations connected therewith. These organized apperceptions and associations insure truer and prompter apprehension of the difficulty to be met and better and prompter selection of the means to meet it. Skill is largely, though not wholly, in proportion to knowledge, and knowledge in proportion to experience.[3]

The player's progress may be divided roughly and for purposes of description into five stages. (1) The first step is to learn the names and movements of the pieces. The former is easily done, but the latter requires a trifle of practice before the pieces can be readily used in play. This is especially true in the case of the knight.[4] For successful play the moves must, in the end, become automatic, and this automatism is not reached, as the game is usually learned, in the first stage itself. It depends for its perfecting on the practice obtained in succeeding ones. This probably is the natural method in all learning, the greater interest of the advanced stages floating the learner over the drudgery necessary for complete perfection of the automatisms of the earlier. When the moves have become automatic the men are no longer pieces of wood, jade or ivory,—static things—but *forces* capable of being exerted in definite directions.

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[1] These last may not seem absolutely essential in view of the case to be described in the appendix, but even from that case I shall hope to show that this statement is justified.

[2] Stout: Manual of Psychology. N. Y., 1899. pp. 84 ff.

[3] I say "not wholly in proportion to knowledge," because skill represents only that part of knowledge that can be readily and effectively applied. Our general problem in this section is, therefore, to describe, as far as we are able, the way in which experience becomes transmuted into skill. Our immediate concern is with chess skill, but if we are successful in our study of that, we shall be justified in certain inferences with regard to many other sorts of learning which, like it, are matters of mental as opposed to purely physical training.

[4] Knowledge of checkers is at first a source of many interferences. The player is tempted to move his pawns diagonally, has a tendency to keep his pieces bunched so that his opponent cannot "jump" them, is on the lookout for vacant squares on which to plant his pieces, and has a tendency to clear the board as soon as possible. He also finds it difficult to remember that the pieces can retreat after having been once advanced.

(2) The second stage may be characterized as the stage of individual moves of offence and defense during which the beginner plays with no definite aim other than to capture his opponent's pieces. Even this he blunders about, often overlooking for several moves a chance to capture a man that has been left *en prise.* My notes contain many entries showing two bishops, both unprotected, left facing each other for several moves, or a queen moved within range of a bishop or a knight. The player is able to attack one of his opponent's pieces and is able ordinarily to defend himself against direct attacks. Whichever he attempts to do he must give his whole attention to it, and even with this extreme of concentration he is able to see only the immediate consequences of the move. In general, however, his lack of conception of the aim of the game, causes him to play at random. His play lacks unity and the pieces are moved hither and thither, unsupported and unsupporting; he has no conception of the game as a well planned sequence. Nevertheless he has hovering in the background of consciousness some idea of the ultimate object of the play, the hemming in of the adverse king, and is influenced somewhat by it.

(3) The beginner is soon able to tell at a glance what any single piece can do, but no one piece, not even the queen, is very strong unless supported by others. Hence the task in the third stage of the beginner's progress becomes that of learning the strength, not of individual pieces, but of pieces in relation to each other. He has to learn the value of groups and the value of individual pieces as parts of particular groups. There are times when a bishop or a knight or even a pawn may be so situated that its direct influence is greater than that of a rook or a queen. Many of the most fascinating of the recorded games are those in which one player has actually given away one or more of his pieces, often his queen, in order to gain the advantage of the relative positions resulting from the movement of the pieces involved.

About the time the beginner has passed beyond the first two stages of his learning and during the third, the idea of checking becomes the dominating one with him and his efforts tend to centre upon that exclusively. This, of course, leads to premature attacks which usually result disastrously to the aggressor. He is also prone to fix his attention on his own plans and most likely on the particular part he is about to execute at the moment, to the neglect of all others. He suffers from inability to take in a number of details at the same time. They have no meaning except as details, and if he concentrates on one, others must, by that very fact, be neglected. He has ittle idea of the importance of developing his pieces, *i. e.*, making them available for future offence and defense, and of the value of position. The attack of his opponent compels some defensive play, however, and no defense can, of course, be made without the co-operation of at least two pieces, so that he soon learns something of the use of pieces in combination. He learns, for example, that often a piece may defend another and at the same time attack one of his opponent's pieces, that in some cases where two pieces are attacked simultaneously one may be withdrawn and so placed as to protect the other, and that a counter attack is often the best defense.

He has made considerable progress in this stage when he is able to give attention to the plans of his opponent beyond those that are immediately connected with his own, though in this particular, temperament plays a large rôle. That this is the usual experience, however, is testified by the fact that after a player is able to form a definite plan of his own involving some use of combination, he is often surprised by checkmate when he is within a single move of checkmating his opponent. He is unable to carry out his own plans and at the same time to give attention to anything else; he is particularly weak in defense.

In general we may say that the beginner at this stage is not able to play in proportion to his knowledge. He recognizes his errors when they are pointed out to him, but he is unable to avoid them. My records show many familiar blunders occurring over and over again. The beginner's material of knowledge is not organized and therefore not available in any situation except the most simple.

(4) The player has entered upon the fourth stage when he begins consciously to plan the systematic development of his pieces. This necessarily involves some knowledge of the value of position, which knowledge we may call judgment of position. These judgments are generalizations and are the result of the player's own experience, or have come to him in the form of general principles from the experience of others. However they may come to the player their possession is absolutely essential to further progress. Now the player no longer has to puzzle himself by attempting to consider all the possibilities of the situation, a thing he is utterly unable to do, but he applies his principle. His principles, especially those he has formulated for himself, are usually only partially true and have to undergo constant modification as his knowledge and experience increase. He knows now a number of definite situations and his plans radiate from these and are more far reaching. He is also in a position to give more attention to the moves and, indeed, to the general plans of his opponent. This is a consid-

erable advance, for it means that the player's mental horizon has been extended very much and that he is able to disregard the non-essentials to a greater extent than before. Given positions assume more and more importance and one of the great marks of improvement is the development of "position sense."[1]

(5) As we have already pointed out, "position sense" is a result of experience, and as such is the product, we may almost say the culmination, of one's whole chess development. Nevertheless, a fairly good knowledge of the value of different positions marks such an advance over the player who is in what we have called the fourth stage that it may be taken as the fifth in the player's course of development.

The stages mentioned above are somewhat arbitrary, and may not be followed exactly in individual cases, but they will at least give some indication of the course of the player's development, which may be summarized in brief as follows: First the names and moves of the pieces are learned. Then comes the period of blunders, of indefinite play, of premature attacks, and of concentration on single moves, particular situations or, at best, on a plan imperfectly worked out. Later, one is able to see farther ahead, to foresee results more accurately, and to give some attention to the plans of his opponent. At the same time some typical forms of attack and defense and some general principles, or supposed principles are being learned, together with some knowledge of position. Along with all of this, though appearing consciously much later, goes an ever increasing power of analysis and improvement in "position sense."

Some of the most common blunders or oversights of these early stages are leaving pieces *en prise*, *i. e.*, unprotected and in a position to be captured on the next move of the opponent; allowing two pieces to be attacked simultaneously by one piece; removing a guarding piece, resulting in the loss of the guarded one; allowing a piece to be "pinned," *i. e.*, leaving it in such a situation that either it cannot, under the rules, be moved at all, or only with loss of an important piece. Errors of a more general nature are overlooking the bearing and force of distant and far-reaching pieces, errors in pawn play, not correlating the pawns and pieces, blocking the radius of action of the men, forgetting the purpose which prompted the placing of a piece in a certain position and a consequent loss of time in replacing it, or a disorganization of forces, and finally, faulty combinations and unsound sacrifices. Many blunders arise at all stages of skill from haste, impatience, and impulsiveness, but they are especially numerous with beginners.

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[1] *Vide*, p. 277.

2. *Discussion of the Learning Process.*

We have now given an account of the stages of learning; it remains to speak more particularly of the psychology of the learning process. Our problem is to explain the development of a beginner, who knows merely the names of the pieces and their powers, into the skillful player who makes use of these simple elements in intricate and purposeful combinations. We have to do with the growth of skill in strictly mental operations within the limited field of chess play.[1]

Obviously, memory is the *sine qua non* of learning, but although of prime importance it is only one of the factors involved. It must be such a memory as leads to the organization of the mental materials rather than to their mere retention. One could not be far wrong in saying that mental skill is in direct proportion to the degree of this organization. How organization can best be brought about is still an open question, and indeed its answer would involve the entire psychology of pedagogy. Its ultimate nature we do not know. To a great extent the material organizes itself, *i. e.*, the organization is physiological and a matter of growth. This fact was clearly pointed out by Dr. Burnham, who holds in his study on "Retroactive Amnesia"[2] that impressions require a certain time in which to fix themselves. The growth process, fixing the impression and strengthening the association tracts, is an indispensable factor in learning. A multiplicity of impressions might be made to follow so closely on one another that none of them could become fixed. In that case, of course, nothing would be learned.

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[1] Numerous studies have been made on memory, attention, and other complex mental processes and a considerable number on learning, but these latter have been concerned chiefly with motor training. Bryan and Harter's study on the learning of the telegraphic language among the earlier studies, is the nearest approach to the present one, but it deals more especially with a sort of learning which is of a mixed motor and sensory type, whereas the skill here in question is almost wholly central. In that study learning on the sensory side consisted in the formation of fixed associations between complex sounds and the corresponding words; in our case the learning process involves the formation of complex groups rather than that of fixed associations of symbol and word. Nevertheless much of what Bryan and Harter discovered in reference to this latter sort of learning is strictly applicable to the form with which we are dealing, especially their chief generalization, namely, that advance in skill depends upon the formation of a "hierarchy of habits." Among the more recent studies, that of Swift, on Beginning a Language, in the Garman Commemorative volume (Studies in Philosophy and Psychology by former students of Charles Edward Garman, Boston, 1906) may be mentioned as dealing like this with a form of mental skill.

[2] Burnham: Retroactive Amnesia, *Amer. Jour. of Psy.*, Vol. XIV, 1903, pp. 382-396.

In this connection I may mention that the returns of my correspondents also indicate that short periods of rest from chess practice, varying with the individual from a few weeks to several months, may cause a noticeable increase in skill. Renewed interest and consequent greater effort in beginning again after an interval of no play may account for this in part, and it may be also that in constant playing the details accumulate faster than the mind can assimilate them, so that they confuse rather than aid the player. This seems plausible when we remember the difficulty the beginner has in applying known principles to a mass of details. Then, too, when the stress of new impressions ceases, an opportunity is given to take an inventory of the mental stock. This is not possible to any great extent when new impressions are crowding in, and the attention is fully occupied with them. On the other hand, long periods of inactivity have a very different effect. Players make blunders in the openings, their combinations are not so far reaching, and a greater effort is required. Every part of the game that requires pure memory is affected and it is often necessary to do consciously what had previously been automatic. This, however, has to do merely with the fixation of separate impressions and of ideas with their associates, and our problem is rather to account for the combination of these elements into larger and larger complexes. On the physiological side little is known. The most that can be said is that increasing complexity of nervous function parallels increasing complexity of mental function. However that may be, our explanation, for the present at least, must be sought on the psychological side.

If we omit the very earliest stages in the chess player's development, the first significant fact is the beginner's utter inability to use in actual play what little chess knowledge he possesses. His blunders are recognized at once when they are pointed out to him, but in spite of his resolution to avoid them, he finds himself committing the same ones over and over again. It seems that the more he tries to avoid them the more blunders he makes. The intensity of his effort and the deep interest he takes in the game precludes mere carelessness. His difficulties are not due to lack of attention, but to the concentration of the attention on one feature of the game to the neglect of all the others. He sees this single thing and nothing more, because it, of all the mass of impressions, has some meaning for him. Were it possible to determine the span of one's chess attention during the different periods of his progress in learning, it would be possible to give objective evidence of the progressive fusion of the different elements into larger and larger complexes. The course of development would extend from the stage in which the player is unable to see in their completeness even the immediate consequences of a single move to that in which he is able to take in at a glance the disposition of all the pieces on the board. The building up of mental complexes in learning chess and those involved in other sorts of learning are not essentially different. There is a close analogy, for example, between the chess player learning the moves and blundering through his first few games, and the child learning to read, or the telegrapher learning to send and to receive messages.[1] The letter, the telegraphic dot or dash, or the single move in chess is at first the unit of perception. Later the word, a series of dots and dashes, or the relation of two or more pieces to each other becomes the unit. The child learns later, possibly, to comprehend at a glance the meaning of a phrase or a sentence; the telegrapher to receive by phrases; the chess player to take in a whole situation at a glance. Not only has the unit of perception become larger and larger but it has become more and more meaningful.

Perhaps the anology is closer still between the chess expert and the mathematician who has merely to glance at a formula or at its first two or three terms in order to recognize its full import. Every situation in a game of chess which requires readjustment of the player's plans is a problem for him, and the quickness and the accuracy of his solution will depend upon his ability to seize upon the salient and essential features and to neglect those which have no meaning for that particular situation. Obviously the mathematician's skill, when confronted by a problem, will display itself in his ability to recognize the fundamental nature of the problem. Lindley found that an expert mathematician, among those who attempted to solve his puzzle, recognized at a glance the mathematical principle involved and solved it without difficulty.[2] He displayed what corresponds to "position sense" in chess. The chess player has this advantage. In any particular game he has built up or helped to build up his own problem and has a mental record of its progress. He has seen the possibilities ot certain lines of play eliminated one by one and is thus able to concentrate on the remaining ones.

The expert chess player is not required to analyze each position as he comes to it, and, indeed, this would be impossible to any great extent. His mind grasps the situation as a whole and it has a definite meaning for him. He recognizes the salient features only and deals with them, the details having for the time being dropped out. He is in the position of the general who has to know not that in one part of the field he has a regiment of one thousand soldiers, divided into ten companies of one hundred men each, but that he has a force there sufficient to repel any ordinary attack. He has only to pay attention to the regiments and their condition when an emergency arises. The expert no longer deals with particular terms, but with general terms or concepts. These general terms have been built up step by step, their meaning changing with the ever increasing knowledge of the player, and are often represented partially or symbolically by their initial moves or general trend. More concretely, a player learns at first that a certain move is a good one because it has certain definite advantages, and this enables him to plan a little further ahead. Later he finds that this move has a great many other consequences, and perhaps this in turn modifies a general principle he may have based upon it, and this finally, may involve the modification of several other principles and result in a still more comprehensive principle embracing all of the others. Details can be organized into larger groups in proportion as they gather meaning as a group, but not before. The chess player groups his pieces and they acquire a meaning analogous to the potential meaning of the general term or the symbol in abstract thinking. Progress in chess like progress in abstract thinking of any other kind consists in the formation of an increasing symbolism which permits the manipulation of larger and larger complexes.

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[1] Bryan and Harter: *op. cit.*

[2] Lindley: *op. cit.*, p. 470.

We are in the habit of speaking of the automatic in the motor realm, meaning by it that certain movements or combinations of movements are carried on without conscious guidance. Is there such a thing as automatism in the realm of the purely intellectual? It seems to me that this question is to be answered in the affirmative. There is something in the purely intellectual life corresponding to motor automatism, which is shown in the ability to think symbolically or abstractly, and thus to handle large masses of detail with a minimum of conscious effort. It involves the increasing ability to take in during a single pulse of attention a larger and larger group of details which means, of course, that the attention is no longer needed for each one.

An apparent difference between motor and mental automatisms, lies, however, in the fact that in the intellectual realm increasing automatism seems to involve the dropping out of details, while in the motor realm increasing automatism often means a greater perfection of the details. Careful examination, however, will probably show that in both details are dropped from consciousness and that in both they are perfected in the externalized outcome. The great feature common to both is the releasing of the attention from the details. In the intellectual sphere, as the processes become more and more complex, they are carried on by systems of symbols which tend to become more and more abstract or general. This is true of all abstract thinking, including that involved in expert chess playing. And, as in all other kinds of abstract thinking, it is essential in chess that no matter how symbolic the thinking may become the player must always have a thorough grasp of the details of the game. In other words, he must not only be able to construct his plans by the use of abstract symbols, but he must be able to translate them into the concrete and to carry them out move by move. This latter he does not necessarily do in his thinking. From one whole situation he passes directly to another whole situation. For instance in a definite situation, the first move of a long series suggests not the next move but the position after the whole series has been played. In other words, the first term does not necessarily call up the second one or the last, or some intermediate term, but the result of all the moves. This final result may be present to the mind in the form of a visual image (a mental picture) or in verbal terms. For example, the first few moves of the Evans gambit already mentioned, may cause to arise in the mind of one player a visual image of the position as it will appear after a dozen moves have been made on each side, while in the case of another player a verbal judgment of the strength or weakness of the final position may take its place. To the latter player this opening calls to mind a verbal judgment of the final position based on past experience. The formulation of principles of play, which become increasingly general, is another expression of the increasing symbolism involved in learning to play chess, but in this case in verbal instead of visual form.

The chess player's skill is measured largely in terms of his ability to use larger and larger units of thought. He has learned by means of many repetitions, a series of moves in regular sequence, later, as has already been pointed out, the first move or a given arrangement of the pieces on the board represents for him the position as it will be several moves further on. All the intermediate steps are for the moment ignored, or, in other words, "a short circuit" has been established and the association is between the first term and the last or the total result instead of each term being revived by the one immediately preceding it.

In trying to explain this from the physiological side two alternatives present themselves. It may be that an entirely new brain tract, connecting the first term with the last, has been opened up. On the other hand it is just as conceivable that the nervous impulse may travel along the same path in all cases and that in the case of a "short circuit" only the first and last terms rise into consciousness. Experiments on the learning of nonsense syllables, showing that repetitions not only strengthen the associative bonds between a syllable and the one immediately following it, but also between more remote ones,[1] seem to lend a certain support to the latter theory. This is, however, all rather speculative since neurology is able to tell us little or nothing about it. On the psychological side the "short circuiting" process seems to mean something like this. In the beginning the last term, the final result, is reached after passage through all the terms of the series. Now, ordinarily, the series is of value, and therefore of interest, not for itself, but for its result, so that little attention is given to the intermediate links, but much to the getting through. The whole strain of attention is forward. As a result of this the image before the mind may be several steps in advance of the one actually being executed, or, in well practiced series, it may be the last step itself, or even the purpose for which the series is gone through. The result is that there is a tendency to the formation of immediate associations between the earlier and later steps of the series. This suggests that conscious effort plays an important part in the establishment of the "short circuit." Bryan and Harter, in their study of telegraphic language, concluded that only by putting forth a supreme effort could one rise above the plateau of moderate attainment.[2] Still it is by no means certain that the rise in the curve would not take place in time if effort were maintained at a moderate and uniform level. In that case the rise in the curve from the plateau would mean the completion of the growth processes under the guidance of ordinary selective attention.

While chess is a type of purely intellectual learning, the fact should not be lost sight of that the emotional accompaniment is an important factor in the chess player's development as in all other sorts of acquisition, and that emotion is one of the strongest influences in fixing impressions. Ideas which are associated with strong emotions are kept before the mind for a longer period than those which have little or no emotional coloring and thus have much more chance of becoming permanently fixed. Numerous instances were noted in this study in which situations which had aroused strong emotions were continually before the mind and were so persistent as to banish sleep and to drive out all other thoughts.

In this connection mention should be made of the effect of error on one's progress. If one continues to commit errors through ignorance of the fact that they are errors, he may retard his development by falling into fixed habits of unsound play; but if they are noted as errors, and especially if they arouse a strong emotion, they are eliminated. The importance of this becomes evident when we recall that a great part of the player's progress consists in the elimination of unprofitable moves. It is easy to see, also, that emotions, so far as they are expressive of temperament and affect one's habits of play, may exercise an important influence for good or bad upon one's ability to win, as already pointed out in any earlier section.

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[1] Ebbinghaus: Ueber das Gedächtniss. Leipzig, 1885.
[2] Bryan and Harter: *op. cit.*, p. 50.

3. *Aids to Learning.*

By study and practice the difficulties of the beginner are gradually overcome and his faults corrected, though the latter are apt for a long time to recur at unguarded moments, and some, especially the faults of temperament (errors and oversights due to impulsiveness, rashness and quick temper, for example), may never be wholly suppressed. It is probable, indeed, that most of the faults of the earlier stages are temporarily overcome many times before they can safely be given over to the realm of the automatic, *i. e.*, they crop out from time to time when the attention is turned toward larger complexes of elements.

Of all the aids to learning, so soon as one has mastered the bare rudiments of play, there is probably nothing like actual play over the board, provided that one is willing to play slowly, study out the causes of his misfortune and profit by them. The emotional stress attending both success and failure at such a time is a great aid to memory, as has already been suggested.

The concrete criticism of a superior player is of the greatest assistance, but too many things must not be given at once, and what is given must be applied immediately in actual play in order to insure its retention.

In order to get some idea of the sources from which chess players gain their knowledge of the game and the value which they attach to them, questions were asked of my correspondents in regard to the benefit derived from problem solution, the study of standard games, end games and openings, and board play under different conditions. Most, of course, had derived most of their knowledge from actual play over the board.

The interest in problem solving is by no means universal. Many state that they have never attempted to solve problems; others, that they are not interested in them because they are artificial and mechanical and do not help one's general play. The replies indicate, however, that problem solving is widespread among players. As to its helpfulness in general play, the variety of opinion is great, varying from the statement that it is a positive detriment to extravagant claims for its utility for mental development in general. With a number of players, the problem interest, if developed at all, was developed late, *i. e.*, long after they had learned to play. It is interesting to note that few of these players think that problem solving has helped their play. Others took up problems with the beginning of play and say that they were greatly helped by their efforts to solve them. This suggests what is probably the fact that solving problems helps one in the early stages of his play, and this is in accord with my own experience. The reason for this is not hard to find. The history of problem chess shows that in the beginning the problems were merely positions taken from actual games and consequently involved all the elements of actual play. Much could then be derived from their solution which would be of general service. Since that time, however, problem composition has changed very much, and the problems now are made to conform to certain fixed rules, which have, from the standpoint of many players, made them mechanical and artificial. They have lost most of their resemblance to positions met with in actual play. No doubt they are not of much benefit to players who have had considerable experience and who are familiar with the principles involved in their solution. With the beginner, on the other hand, the case is different. He may learn something of the manner of giving check, something of the powers of pieces in different combinations, and of the value of position. They may help his powers of analysis in so far as they involve general principles which are applicable to actual play, and they may aid in improving his judgment of position. At the best, however, they are far inferior to the study of end games and to actual practice over the board. This latter statement seems borne out in part by the fact that few, if any, great problem solvers or composers are also great players.

Practically all agree that a knowledge of the openings is indispensable. The advantage is evident. It enables one to place his pieces in good positions relative to each other, to develop along sound lines, to avoid disaster in the early stages of the game, to take advantage of weak moves made by one's opponent, and what is also of great importance, it enables him to play with a minimum of effort during the early stages of a game. It should be added that knowledge of the openings and variations helps one to force the play along lines with which he is most familiar. The easiest and quickest way to get this knowledge is from the books, but many good players possess it who have given little or no time to book study. They have gained it from actual experience, and base their opening plays on principles derived from this source.

A few think that replaying standard games does not help one's play, and a still smaller number think it is a positive detriment, assigning as a reason that it destroys one's originality, and causes him to overlook advantages which slight variations from the known lines might give. There may be a real danger here, but it is more than offset by the advantages gained. Among the advantages are mentioned the opportunity to examine positions at leisure, to study comprehensive plans of attack and defense involving particular combinations, to appreciate the value of time and position, and finally to become familiar with a number of oft-recurring situations. These situations, while seldom identical, are often similar. Standard games also teach principles and aid in the development of position judgment. It should be stated, however, that the value of such games varies with the individual, and up to a certain stage is in direct proportion to his chess knowledge. The mere beginner learns little from them; the chess master also learns little from them. The one is unable to comprehend them; the other finds little in them that is unknown to him. The games take on meaning in direct proportion to the amount of knowledge that one brings to them; and their value to any individual depends on the number of new ideas he is able to carry away from them.

Playing with a weaker player is not considered a good thing by most of my correspondents. They say it makes them careless, prone to recklessness, and leads them into all sorts of extravagancies of play. Several recommend never playing with a weaker player without giving sufficient odds to make the game even. A few recommend playing with a weaker player for the reason that, by lessening the amount of attention ordinarily given to the opponent's plans, the stronger player is able to give freer play to his imagination than he would dare to do if playing with one of equal strength.

Most say that playing with many different players has made their style more flexible. A few, however, maintain that style of play is individual and that nothing can change it. This contention, as was pointed out above, is undoubtedly true in so far as fundamental traits of character enter into the game. Those who answered that playing with a number of different players has made their style more flexible, appear to mean that to a certain extent it has enabled them to overcome some faults due to temperament and that they have learned a greater variety of methods of play.

III. GENERAL SUMMARY OF THE PSYCHOLOGICAL POINTS.

Chess as a strongly competitive form of human play appeals to the fundamental fighting impulse, but it appeals also to the æsthetic and puzzle-solving interests; and it affords the pleasure of "being a cause."

Visual imagination is an important element in chess playing, especially in blindfold chess, but it is not indispensable. Motor, verbal, or auditory imagery may, and often does, occupy the chief place in the player's consciousness.

The mental qualities most utilized in chess playing are: a strong *chess* memory, power of accurate analysis, quickness of perception, strong constructive imagination and a power of far reaching combination. These are *chess* qualities, however, and skill at chess is not a universally valid index of high mental endowment.

The logical type differs in the different stages of a game and with the knowledge and skill of the player, approaching always nearer, as his knowledge and skill increases, to that of the syllogism.

The reasoning process is, in psychological terms, a sequence of mental states due to shiftings of the focal point of attention, the associations working strictly within the limits imposed by the task or purpose.

In his learning the chess player passes through well defined stages and these mark the necessary steps in his progress. The most important psychological feature in the learning of chess (and it seems equally true of all learning), is the *progressive organization of knowledge*, making possible the direction of the player's attention to the relations of larger and more complex units. The organization involves generalization, increasing symbolism, and the multiplication of associations; it insures prompter recall and increased potential meaning in the general concepts; it releases attention from details and favors consequent mental automatisms and "short circuit" processes. Thus alone is progress possible. Mental automatisms are usually perfected, one may conjecture, after advance to the next higher stages of learning.

Appendix: On the Case of a Feeble-minded Chess Player.

During the course of this study several cases of chess playing among the feeble-minded have been reported to the writer, but it has been impossible to secure definite data except in one case. It is said that in some instances a very high degree of chess skill was possessed by men of very low mentality. An inmate of the Wisconsin Institution for the Feeble-minded, is reported to have been able to cope successfully with very strong players. Very likely the strength of these players has been very much overestimated, but the evidence is sufficient to warrant us in saying that in chess as in other kinds of mental activity a peculiar power is not incompatible with a very low average of general mental ability.

The writer has been able to study at first hand one case of chess playing by a man of low grade intelligence who is an inmate of the department for the feeble-minded and criminally insane at the Massachusetts State Farm. In the asylum records he is classed as a congenital idiot who has suffered degeneration since coming to the institution in 1891. Previous to that time he had been an inmate of other institutions for the insane. He has had and still has, though less frequently than formerly, outbursts of rage, at which times he beats his head against the wall. He says he does this because he loves his mother. He is a sexual pervert and some of his outbursts followed his separation from other inmates of the institution whom he designates as "friends." He is fifty-four years of age but looks much younger, is filthy in his personal habits, and presents a very peculiar appearance. He stoops considerably and walks with the shuffling gait characteristic of the feeble-minded. In one of the older asylum records some one has noted the fact that he resembles an anthropoid ape in appearance. His forehead is very low and receding, his maxillaries are very protruding and the posterior portions of his head are so prominent that his head resembles that of the African negro.

The term idiot is used to cover such a wide range of mental deficiency that it conveys no very definite meaning, so that it will be necessary to give a brief account of his attempts at mental work in order to convey some idea of his general intelligence. His memory for some things is fairly good, though it is not of special excellence. He remembers faces quite well and for a considerable time. He also has a fairly good memory for places, remembering, for instance, the town in which he was brought up, the different institutions he has been in, and the town in which some of his relatives live, and remembers all these by name. He has no idea of time, but holds a few dates in mind. For example, he said he came to the asylum in 1891, which was correct. He knows the names of most of the months of the year, but has no idea of their order. In January he was asked what month it was and replied that he did n't know. He was then asked if it were June and replied that it was the month before June. When asked what month that was, replied: "That is the month of October." He has had practically no schooling and can neither read nor write. When asked why he didn't go to school when he was a boy he replied that he was too thick-headed to learn. He repeated this on several occasions.

The following questions were asked him: If you had two apples and I gave you two more how many would you have then? How many are five times five? If you worked for me five days and I gave you a dollar for every day you worked, how many dollars would you have, To all these and to other questions he gave the same answer: "Don'? know." Questions in regard to his name, the names of otherst his age, and other simple questions he answers intelligently or with his indifferent "Don't know." In this regard he may be compared to a young child. There is this difference, however: he does not show the curiosity of a child, and displays very little mental iniative. He is like a child, however, in another respect: he is very fond of toys, picture books, and especially of neckties. He asks for them repeatedly, but only apparently when he notices them. He enjoyed playing with my watch and with my ring and asked for the latter several times. When told he could not have a thing or promised it later he always replied "Thank you." He is unable to tell time by the clock or watch, but almost always knows the hour of the day, which he is no doubt able to determine from the regularity of the institution life. In reply to a question he said that he is twenty-one years old and that he had been that age for a long time.

In regard to his chess playing I should say at the outset that he is not a strong player, and that an average player of a year's experience could probably play as well or better. It should be remembered, however, that he has never studied the game at all, has never played regularly, and has not played with many different players. There was no way of determining how long he had known the game, except from his own statements and these are, of course, not very certain. He said he learned about three years ago, that no one had taught him the moves, but that he learned them by watching others play. He has played checkers for many years, but there is no trace in his game at present of interference of association from this source. As is to be expected from the circumstances under which he learned, and played, his play shows very little variety, although there was some improvement in this regard as well as in general chess ability during the time I had him under observation. He has considerable familiarity with certain situations and can be relied on to meet them in certain ways. He usually meets a threat, for example, at once and by dislodging the threatening piece if possible. An analysis of his games shows a number of oft recurring moves such as Kt-R3, Q-B3, P-Q3, and advancing a pawn one square to serve as a guard for a piece or a pawn to be advanced at the next move. Attacking a piece with a pawn, and "forking" two pieces are favorite methods of attack with him. He makes his moves very rapidly and apparently with little or no time for consideration, but usually waits very patiently for his opponent to reply. If the effect of a move of his opponent is not very remote, he notes it almost immediately. For instance, on one occasion when a bishop attacked both of his rooks he announced at once that one of them was lost, and on another occasion when his queen was attacked by a knight, he announced at once that she was lost, a fact which his opponent had not yet appreciated. It may be, of course, that he had anticipated the dangerous move.

He had a great deal of difficulty with a set of chessmen of a pattern different from those he had been using. In the new set the king was larger than the queen, while in the old set the reverse was the case. He was utterly unable to use them until, at his request, a piece of colored cloth, which had been tied around the old queen, was fastened to the new one. After that he had little difficulty with the new set.

At times he seemed to see a situation very quickly, but to be unable to retain it in mind when he attempted to meet it. For instance, when trying to get out of check, he moved his king back into check several times, that is, he would find a move impossible, recall it and then a little later attempt it again.

On the whole it is not too much to say that his game compares quite favorably with those of players whose advantages in the way of instruction, study, and practice have been much greater than his, and there is no reason to doubt that with more practice and instruction he would be able to improve his game considerably.

Our conclusions from the study of this case must be, it seems to me, that chess skill is not an index of general intelligence, that the reasoning involved in chess playing is reasoning within very narrow limits, and that a considerable degree of chess skill is possible to one who is mentally deficient in almost every other line.

The following records of games played by this player will indicate to those who are familiar with the game something of his chess ability. The games are chosen as fairly representative of his play during the time he was studied, which extended a little over two months, with an interruption of three weeks between the last observation and the one just preceding it.

White (feeble-minded player)	Black.
1 P-Q4	P-Q4
2 P-K3	Kt-QB3
3 Q-KB3	P-K4
4 P-QB3	Kt-B3
5 Kt-KR3	QB-Kt5
6 Q-Kt3	Q-K2
7 P-KB3	B-R4
8 QPxP	KtxP
9 P-KB4	Kt-QB3
10 QKt-R3	KKt-K5
11 Q-B3	BxQ
12 KtPxB	Q-R5ch
13 K-K2	Kt-KB3
14 KKt-Kt5	KB-B4
15 Kt-QKt5	O-O
16 P-QR3	P-KR3
17 P-Kt4	B-QKt3
18 KtxKBP	RxKt
19 P-QR4	P-QR4
20 PxP	BxP
21 K-Qsq.	R-K2
22 B-Q2	B-Kt3
23 B-Ksq.	Q-R4
24 KR-Kt	QxBp (ch)
25 K-Q2	QxP (ch)
26 K-Kt2	QxR
27 Kt-Q3	QKtxKt
28 PxKt	BxQP
29 R-Q1	QxKB
30 B-Kt4	Q-QB5 (ch)
31 K-Ktsq.	Q-Kt6 (ch)
32 K-B	32 Q-Kt7 (checkmate)

Game No. 2.

White	Black (Feeble minded player)
1 P-K4	P-K 4
2 Kt-KB3	Q-KB3
3 P-Q3	Kt-KR3
4 B-Kt5	Q-QB3
5 KtxKP	Q-K3
6 B-KB4	P-KB3
7 BxKKt	PxB
8 Kt-QB4	Q-QB3
9 KB-K2	P-QKt4
10 B-KR5 (ch)	K-Qsq.
11 P-Q4	QxKt
12 Kt-QR3	Q-B3
13 P-Q5	Q-Kt3
14 O-O	B-QB4
15 Q-K2	B-QR3
16 P-K5	P-QKt5
17 KPxP	BxQ
18 R-Ksq.	QxP
19 RxB	QxQKtP
20 R-Ksq.	QxKt
21 R-K8 (ch)	RxR
22 RxR (checkmate)	

FLUCTUATIONS OF ATTENTION TO CUTANEOUS STIMULI.[1]

By L. R. GEISSLER, B. L.

Ever since the term "fluctuation of attention" came into psychological usage, it has been assumed by many psychologists that attention itself is an intermittent, oscillating, more or less rhythmically pulsating condition or state of mind. A sufficient proof of this was thought to be found in the fact that a continuous liminal stimulus periodically enters and leaves consciousness. On the other hand, it has also been shown by several investigators that the unsteadiness of liminal sensations may be due to various other causes. This would not necessarily disprove the oscillatory nature of attention. But the results of an investigation carried on during the last year tend to show that, under certain favorable conditions, maximally concentrated attention to liminal and supraliminal cutaneous stimuli remains approximately constant for at least 2 to 3 minutes, provided that physiological adaptation of the sense-organ and violently intruding external or subjective distractions can be prohibited for this length of time. This is the general outcome of a series of experiments suggested by the incongruity between the results of previous investigators of fluctuations of attention to cutaneous stimuli.

The first experiments of this kind were made in the year 1887, by N. Lange, with a Du Bois-Reymond induction coil.[2] The observer held one of the electrodes in the one hand and placed a finger of the other hand in a vessel of lukewarm water containing the other electrode. Lange found that fluctuations in this sense department were not as plain as in the case of visual and auditory stimuli, and in fact could be observed only after some practice. He thought the reason for this was that in daily life we are hardly ever required and therefore little accustomed to direct our attention to tactual sensations. In 1892 similar experiments were made by A. Lehmann, who also compared the occurrence and duration of electro-cutaneous fluctuations with the periods of concurrent respiration.[3] He found a surprising coincidence, which led him to the conclusion that the periodical innervations of the muscles, caused by inspiration, in some way increase the sensitivity of the skin.

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[1] From the Psychological Laboratory of Cornell University.

[2] Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception. *Philos. Stud.*, IV, 1888, 390 ff.

[3] Ueber die Beziehung zwischen Athmung und Aufmerksamkeit. *Philos. Stud.*, IX, 1893, 66 ff.

Observations of an entirely different kind were made by E. Wiersma, who in 1900 and 1901 investigated among other things the fluctuations of attention to areal pressure sensation.[1] He employed a series of 6 circular weights, 1 cm. in diameter, the lightest being 7.4g, and each following 3g heavier; so that the series extended to 22.4g. These weights ranged for him from a liminal to a distinctly supraliminal intensity. Each observation lasted 5 minutes, after which the observer rested for about 8 minutes. Wiersma found that the total time of complete imperceptibility, as well as the duration of the different periods of fluctuation during a single observation, are proportional to the intensity of the stimulus, and that toward the end of each period of observation liminal stimuli, pressure as well as visual and auditory, tend to (and often actually do) disappear entirely. This tendency of conscious processes to lapse into unconsciousness seems to him to be intrinsic to the fluctuations of sensitivity. Quite different were the results of Ferree's experiments, both with liminal pressure stimuli and with liminal electro-cutaneous sensations.[2] No fluctuations were experienced.

Partly for this reason, we decided to repeat his and also Wiersma's experiments. We also wished to obtain introspective data as to the course and contents of consciousness during the experiment, because we expected them to throw light on the conditions of fluctuation. According to Lange and many others, the intermittences of liminal sensations are due to central causes, *i. e.*, they represent fluctuations of attention; in other words, a liminal sensation periodically enters and leaves consciousness because attention itself is an intermittent or pulsating process. Now it may be true that a minimal lapse from maximal to a somewhat less concentrated state of attention is sufficient to cause such disappearance, without our being able introspectively to observe this slip or lapse. But if it is unnoticeable, then there is no justification for calling attention intermittent; if it is noticeable, then introspection ought to bring it to light. So far as our knowledge goes, no systematic appeal to introspection has been made by the supporters of Lange's hypothesis. On the other hand, Lange's opponents have maintained that intermittences of liminal sensation occur in spite of the most concentrated attention, and hence must be due to something else. They have referred this phenomenon to periodical changes in the physiological conditions of the sense-organ or in the sensory centres of the cortex. Introspection would be plainly in their favor if it could tell us, not only what is in consciousness while the oscillating stimulus is subliminal, but also what is the state of attention during the absence of the original sensation; and, especially, whether it is possible for attention to remain maximal while different processes enter and leave its focus during a given period of time. From the 350 introspective records obtained during the course of our investigation, we can, in a preliminary way, draw the conclusion that after some practice it is possible to estimate the degree of attention given to a certain stimulus with fair accuracy, and to notice any shifting of the focus of attention.

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[1] Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen. *Zeits. f. Psych.*, XXVI, 1901, 168-200; XXVIII, 1902, 179-198; XXXI, 1903, 110-126.

[2] An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention. This *Journal*, XVII, 1906, 119.

I.

For the sake of comparison, we copied as closely as possible Wiersma's arrangements and external conditions. Unfortunately, outside noises could not be entirely avoided. But we tried to eliminate at least all visual disturbances or distractions by using the darkroom for each experiment. The fluctuations were recorded by means of a telegraph key, connected with a stylus writing on a kymograph in an adjacent room. About 300 records were obtained from the following observers: Misses M. E. Almy (A), then a Cornell summer student of slight practice in introspection, and E. Murray (M), a very careful and experienced observer, and Messrs. I. M. Bentley (B), the only observer familiar with the problem besides G, the writer, I. Lande (L), W. H. Pyle (P), and R. W. Sailor (S). Observers M, G, L, P, and S were advanced students in the department of psychology who had had general laboratory training. The first set of experiments, with observers A, B, G, and P, was made during the months of July and August; the second set, with observers L, M, P, and S, from October to December, 1906.

Preliminary trials showed that Wiersma's weights were not liminal for our observers. Nevertheless it seemed worth while for comparative purposes to use his material, and experiments were accordingly made with these weights upon P and G on 6 consecutive days in the order given by Wiersma. The experiments were also repeated for G in reversed order. In the case of the other observers, we were content with one or more observations with each of the 6 weights. At the end of each trial the observer was asked to write out an introspective account of the course and content of consciousness during the experiment. In about 165 trials Wiersma's arrangements were exactly followed, in the remainder some modification as to weight, time, or area stimulated was introduced. The latter will be treated separately later on. The results obtained from A, B, G, and P were entirely opposed to Wiersma's. These observers recorded no fluctuations whatever, but stated that the weights could be perceived even if attention were not directed toward them. But this wandering away of attention from the stimulus occurred almost only in the second half of the experiment, that is to say, after about 150 sec. of maximal concentration; for we must remember that each observation, according to Wiersma's direction, lasted 5 minutes, which is undoubtedly too long and too fatiguing. Other characteristics of the introspective records appear in the following quotations:

Observer A. Weight 16.4g. "Seemed so heavy that I had only to think of it as a weight to keep it in mind. It fluctuated in apparent amount of pressure and in degree of attention, but was easily held in consciousness throughout the time."

Weight 13.4g. "Kept weight in mind by verbal ideas and by projecting my mind on the periphery of the weight as the sensation lessened. I allowed it to slip only once, when by comparing the two hands I decided there was no feeling; and I started to press the key and found I was feeling, though very faintly; the movement of the right hand emphasized its freedom from weight."

Weight 10.4g. "It almost escaped me, as I thought about the weights and what they were made of; but I snatched the pressure sensation back just as it began to fade from consciousness. I felt that the weight was too light to be held easily in mind, and I had to think of it more attentively than I should have to think of a heavier one."

Weight 7.4g. "Had by effort from the beginning to keep weight in first level (*i. e.*, highest degree) of attention, because it was so light and fluctuated in intensity so that I could sometimes hardly feel it. Disturbing noises once almost put it out of consciousness; nevertheless it was voluntarily held at second level. I think in words to keep my attention on it—spoken words to myself—and try to banish all other thoughts. Kept comparing the surfaces of the two hands to be sure the weight could be felt."

Observer B. Weight 13.4g. "Slight throbbing of blood in last part of interval. Surer this time of no fluctuations. Interrupting distractions from fatigue in arm. Some associative trains obscured stimulus. A tremendous strain!"

Weight 10.4g. "Heavy. Distinct throbbings of blood in back of hand and index finger. Pressure lighter in last part of interval. As before, I seem to come back to pressure and clear it up after distracting associations. Pressure seems *not* to have lapsed and returned."

Weight 7.4g. "In last part of experiment, bad throbbing of pulse in volar fore-arm caused some distraction. Just at end I was doubtful whether pressure was still present (yes?)."

Observer P. Weight 19.4g. "Attention on weight throughout, and had richer sensations than usual. Pain came in after about a minute. I thought a few times there was a flash of cold. I can still feel sensation after weight is removed."

Weight 13.4g. "Felt weight well. Its sensation changed, a little painful toward end. Many things passed through my mind, staying only momentarily."

Weight 10.4g. "I kept saying to myself: weight, weight, in order to be sure of my attention. Occasionally I felt my arm underneath and sometimes I noticed my breathing."

Weight 7.4g. "A little difficult at times to feel weight. I would become doubtful and attend very closely, the weight at such times then plainly felt."

"A little painful at times and very faint at last. Could hardly tell whether I felt it or not."

The introspections of G, the writer, are perhaps less valuable than the others, although in their main features they differ little from the rest. A few actual disappearances of the lightest weight, due to some unavoidable distractions, occurred in his case.

Summarizing now the observations of A, B, G, and P, we find (1) that a supraliminal areal pressure stimulus applied to the skin will gradually change its original character as to quality and intensity, and either disappear altogether after 2 or 3 minutes or persist as a simple, indefinite, vague, and unpleasant irritation, which is perhaps sometimes confused with a resulting pressure after-image. (2) These qualitative changes or fluctuations of intensity (due to physiological conditions) are noticed only when and presumably because attention is concentrated and becomes more concentrated the fainter the stimulus grows. (3) After sufficient practice it is possible to estimate with fair accuracy the degree of attention given to different stimuli or to the same stimulus at different and especially critical moments. (4) It seems probable that attention can remain maximal even while different mental processes enter and leave its focus, at least for a few minutes. (5) The weights were heavy enough to press the muscles beneath, and also to emphasize the beating of the pulse (B and G) against the resisting contact surface of the weight; these things served either as distraction or as a criterion for the presence of the stimulus. (6) The slightest movement in the upper part of the body increased the pressure to a surprising degree, probably because it involved a slight movement of the stimulated surface of the hand. The last two points serve to emphasize the fact that very close attention involves great muscular strain. It is possible that breathing, under such conditions, periodically causes greater tension and relaxation of the muscles of the chest and arms, and hence indirectly brings about the (often rhythmical: B and G) fluctuations of intensity in the pressure of the weight upon the muscles underneath the skin. Of course these results cannot fairly be compared with Wiersma's, because the stimuli were not liminal; and therefore another set of experiments had to be made for the purpose of getting liminal pressure sensations.

Unfortunately, it was impossible to continue with the same observers, except P, and hence our new observers, L, M, and S, had to begin afresh with Wiersma's series, in order to determine whether for them these weights were liminal or not. At first sight this seemed to be the case, but the introspections proved the contrary.

For example, L recorded the following fluctuations (indicated by the minus sign preceding a number) in the first set of observations taken:

Observer L.

No.	Weight.	Fluctuations in Seconds.
1	7.4g	+ 58–2+179–6+25–4+13–13
2	10.4g	+136–2+11 –9+55–9+44–14+14–6
3	13.4g	+ 69–2+36 –4+42–7+32–7 +47–10+10–16+18
4	16.4g	+ 51–13+52–15+143–4+22

But the corresponding introspections plainly show that the disappearance of the sensation is due to distractions which forcibly claim attention. In a few cases, especially those toward the end of the observation, the organism gradually becomes adapted to the stimulus, which would, we may infer, be lost without recovery, were it not for certain involuntary tremors occurring in the hand or for slight movements in other parts of the body. Nevertheless, it is important to notice that the longest periods of absolute perceptibility in nos. 1, 2, and 4 lasted 179 sec., 136 sec., and 143 sec. respectively, which is characteristic for all other observations with the same stimuli, and undoubtedly indicates that attention was approximately constant and undisturbed during these periods.

In the case of S the few recorded fluctuations are plainly due to distractions of attention caused by such things as "finger ring pressure," "itching of hand," "twitching of an arm muscle," "ache in right shoulder;" by trains of ideas; or by external noises and disturbances. But with greater practice S learned more and more to disregard these distractions and the number of disappearances became less, being finally only 3 during an observation of 5 minutes. This means, of course, that the periods of absolute perceptibility became correspondingly longer, some of them lasting, for example, 99 sec., 110 sec., 180 sec., and 200 sec. S noticed also in his introspections that the quality of the sensation set up by the weight changes, being sometimes "a pricking sensation under the weight," sometimes warm, and toward the end slightly painful.

The results given by M were of a different nature. Finding that the weight when put on the skin gave not only pressure, but also set up a complex of other cutaneous sensations, she attempted to abstract from them and to observe the pressure only. This required a constant analysis and often made it very difficult to decide whether pressure itself was still there or not. At such moments of doubt attention was easily drawn away to some other disturbing factor, and hence gave rise to a number of disappearances of the pressure, while nevertheless the weight could be perceived in terms of some other cutaneous sensation. This will come out in the following quotations:

Observer M. Weight 22.4g. "Numbish, unpleasant feelings; wanted to throw it off, or, sometimes, to move hand in order to see if it was still there and intensify the pressure. When no distinct pressure sensations were present, there was often a residue of vague itching or vibratory feeling, internal. On the other hand, the return of the pressure, as pressure, was always distinct, abrupt, as if weight were newly lowered on skin."

Weight 19.4g. "Disappearance twice correlated with a tingling in thumb, which seemed to swamp other sensations from hand. At other times distraction seemed to be a sensation in head, which pushed to front of consciousness. Many minor fluctuations of intensity, when the feeling was that while the sensation remained, it was temporarily blurred over, partly by the crowding in of sensations from other parts of the body, were not recorded."

Weight 13.4g. "At first easy to hold attention on pressure sensation. Got tired toward end—hard to concentrate on it again. Sensation itself attended to varies very much—pressure part seems to decrease considerably in intensity toward end. Other elements, temperature, tingling, etc., vary also from time to time. Pressing of key seems to bring pressure element abruptly back. Toward end, conscious of pressure of weight only as a vague discomfort over that area."

Weight 10.4g. "As above, very difficult to keep attention on pressure pure and simple—keeps shading off into faint tingling or glow (cold?). Toward end, sensations from weight, which had been vague and elusive for quite a period, came back when I coughed and involuntarily moved hand. These (pressure and cold) remained with hardly a fluctuation for rest of series, much clearer than in earlier part of record."

Weight 7.4g. "Blanks recorded sometimes (as above) mere lightenings and returns of the pressure sensation in its setting of surrounding cutaneous sensations. Sometimes a total shift of attention to other spheres of sensation,—distraction usually visual. Toward end the sensation reappears more as heat or dull ache than as pressure."

In order to test or check these observations, M was given another series of experiments with the same weights, but with the explicit instruction to record only the intervals when there was no indication of the presence of the weight, whether perceived as pressure or in terms of some other cutaneous sensation. The results were strikingly different, for only a few subjective distractions occurred, such as were mentioned before by her as well as by the other observers. It would be interesting to know whether Wiersma found similar phenomena in his experiments, but unfortunately he gives no introspective account of the qualitative nature of the sensations set up by the weights, and hence it is impossible to say whether his observations are of M's kind or not. A general summary of the introspective material obtained from L, M, and S, with regard to experiments on Wiersma's weights only, agrees in all of the main points with that of the first group of observers and hence need not be repeated here. The results plainly showed that Wiersma's stimuli were by no means liminal for either L, M, or S, and hence we decided to continue the experiments with lighter weights.

For this purpose we made a new set of weights, of 6g, 5g, 4g, 3g, 2.5g, 2g, 1.5g, 1g, and .5g, with the same diameter of 1 cm. The observers were L, M, P, and S, and over 100 observations were taken. We also introduced into some experiments certain variations of procedure, by changing the stimulated area, or by shortening the period of observation from 5 to 3 minutes. These changes seemed, however, to have little appreciable effect upon the results or upon the introspective accounts.

In the case of L, for example, whose earlier results were influenced by the interfering tremors of the hand, this source of disturbance was now not strong enough to shift the weights 6g, 5g, 4g, and 3g, so that they should be perceived again after having once become imperceptible. Final disappearance, probably due to physiological adaptation or perhaps fatigue, did not take place with these weights until toward the end of the second or third minute; for example, after 96 sec., 110 sec., 112 sec., 164 sec., etc. But the lightest weights, namely, 2g, 1.5g, 1g, and .5g, disappeared during the first minute and could not be recovered, save when a considerable involuntary movement or "jar" occurred in the lower left arm. This happened twice, perhaps as the result of an uncomfortable position of the arm during the particular experiment. Introspective accounts were again demanded. They show as before that for some time after disappearance attention was extremely concentrated in an effort to recover the sensation, though without success.

The results of S are very similar to L's. The same total disappearance without recovery is here noted. But again there was not the least regularity or proportionality of the period of perceptibility to the intensity of the stimulus. The shortest period of perceptibility occurred with weight 3g, which once disappeared after 18 sec., although introspection showed that attention was maximal, while the two lightest weights could be perceived for about a whole minute. The following figures are significant, as illustrating the fact that attention could be kept approximately constant to liminal cutaneous stimuli for fairly long periods: 94 sec. for weight .5g, 68 sec. and 78 sec. for 1g, 96 sec. for 1.5g, 84 sec. for 2g, 120 sec. for 2.5g, 112 sec. for 3g, 88 sec. for 4g, 122 sec. and 144 sec. for 5g, and 197 sec. for 6g. Again, the introspections in these experiments bring out the fact that the effort to recover the lost sensation by concentration of attention was unsuccessful, and the attempt finally given up, principally because other cutaneous sensations from the lower arm as well as the other common distractions became strong and even unpleasant.

Different results were obtained with M and P. Both observers perceived even the lightest weights during the whole period of 5 minutes. Subjective distractions, such violently intruding disturbances as "jarring of floor," or "cold and ache in fingers toward end," etc., occurred to M with irregular frequency from one to five times during 5 minutes' observation, and were of very short duration. P also mentioned the occurrence of occasional momentary diversions of attention. The difference between L's and S's results on the one hand, and M's and P's results on the other, is probably due to several factors; but the introspections of the latter observers bring out mainly two. In the first place, it was very difficult for both M and P to tell exactly whether the sensation perceived in the stimulated area arose from the weight or not. To quote from introspections:

Observer M. Weight 0.5g. "Very light weight; attention harder to maintain (first two records show complete distractions of attention). Found myself continually tending to attend to the hand as a whole and compare with normal sensations in order to decide whether weight had disappeared; always seemed to find some kind of sensation left."

Weight 1g. "Much the same as above, but I found it more definitely limited to approximate area of weight. Rarely a light contact sensation; but in first part intermittent pulsing sensation as if throbbing of arteries lifted and dropped the weight; then later, muscular tension and weight feeling, deeper and as intensive as far heavier weights."

P's observations, though less explicit on the point at issue, clearly indicate that the complex character of the light weights was not perceived simultaneously, but rather successively, changing, *e. g.*, from "a very delicate touch" to temperature, generally warm, then to pain, and lastly to a "numbish indefinite something." These "different aspects of the weight," as P himself termed them, helped him in judging the pressure of the stimulus. Nevertheless, he doubted the possibility of sensing a very light weight for 5 minutes, and said that sometimes he was not sure whether what he felt was cutaneous and just below the pressing weight, or whether it was something more deeply subcutaneous, or even at a small distance from the stimulated spot. Since M had entirely given up the attempt to analyze the sensational complex set up by the light weights, she was less easily and less often distracted, and employed, like P, every qualitative change in the stimulation as a criterion for judging the presence of the stimulus. The fact that both M and P had great difficulty in observing the actual nature of a liminal areal pressure stimulus seems to be in line with a similar difficulty which Spindler found in determining the nature of the after-effect of areal stimulation.[1]

This article suggested to us the second principal factor involved in the difference between M's and P's results and those of L and S. Since L and S never, M and P, however, very frequently, mentioned a long lasting after-sensation, it is possible that the latter, instead of having the actual sensation from the weight, perceived only an after-sensation which might have arisen after the weight itself had become imperceptible, though still present. At least both M and P often found that even immediately after removal of the light stimulus the previous sensation persisted without changing in any way whatever. However else the difference might be accounted for, it is not further relevant to the general conclusions to be drawn from these experiments.

Summing up the results, we arrive at the following facts and conclusions:

(1) A liminal areal stimulus applied to the skin after about a minute's time loses its original character, and either disappears altogether after the lapse of another minute or two, or persists as a simple, indefinite, vague, and unpleasant irritation.

(2) It is possible for a practised observer, under most favorable circumstances, to concentrate attention for at least 2 or 3 minutes upon an areal pressure stimulus of liminal intensity without experiencing any kind of fluctuations.

(3) Qualitative changes in the cutaneous sensation itself are noticed only while, and presumably because, attention is maximal.

(4) There is a strong tendency for attention to become more and more concentrated the fainter the stimulus grows.

(5) The muscular effort involved in maximal attention to a single liminal stimulus makes concentration during a period of 5 minutes very difficult, painful, and fatiguing.

II.

In repeating Ferree's experiment with electro-cutaneous stimulation of the tongue, we found some difficulty in eliminating the touch, pressure, and taste sensations set up by the electrodes. The best results were obtained by applying a 1% solution of cocaine to the fore part of the tongue, upon which 2 strips of tin foil (Christmas-tree foil), hammered as thin as possible, were laid. The strips were connected with the interrupter of a Du Bois-Reymond induction coil. Unfortunately, the number of interruptions could not be regulated or even kept constant during a single observation, because the screw by which the interrupter was adjusted tended to work loose. The cocaine itself gave rise to certain sensations; but as soon as these began to fade away the tin foil electrodes could be applied, and were generally not perceived for a long time. The movable coil was adjusted to the limen for the different observers, which was at 10 cm. above the zero point for M and G, and at 9 cm. for L. Each experiment lasted 2 minutes, after which the observers rested as long as they desired. Toward the end of the period, adaptation, or perhaps fatigue, set in and caused the sensation to disappear altogether without recovery. About 50 observations were taken with L, M, and G, during January of this year.

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[1] After-Sensations of Touch. *Psych. Rev.*, IV, 1897, 631 ff.

The results were very similar to those with the weights, and agreed with Ferree's observations. It was impossible to avoid all subjective distractions, which were principally of two kinds. First, it is very difficult to hold the tongue perfectly motionless for more than 1 minute. After this interval it was practically impossible to avoid little involuntary movements, which sometimes caused the electrodes to shift or slip off. The other kind of distraction was due to the fact that generally, after about 70 or 80 sec., the exposed part of the tongue became more or less dry, which either resulted in the entire disappearance of the stimulus or caused the electrodes to stick fast to the tongue and exert a pull strong enough to overcome the faint electrical stimulus. In spite of these difficulties, we obtained 10 fairly good observations from each of the three observers, which plainly showed that liminal electro-cutaneous stimulation of the tongue does not give rise to intermittent sensations.

Introspective accounts were demanded after each observation, whether successful or not. The condition of the tongue immediately after the application of cocaine is described in the first three of the following introspections, while the rest are typical accounts of the experiment itself:

Observer L. "Tongue feels velvety when rubbed against the teeth; also there is what might be called a vague sensation of numbness, a sense of restriction."

Observer M. "Tongue (after cocaine) feels as if it had been burned. A thick, soft layer on surface with slight sting underneath. Tender, swollen."

Observer G. "Tongue feels numb, tight, or stretched, slightly rough, sometimes burning, sometimes cold, as if melting ice was put on it; a metallic-bitter taste remains for some time and finally fades away, leaving only a vague feeling of numbness."

Observer L. "Current always faintly felt; much attention required; tongue numb; stronger sensations at times than at others."

"Current weak; fluctuation (*i. e.*, a total disappearance without recovery) I believe due to electrodes sticking as it were to tongue which dried from exposure. During fluctuation attention was still focussed in an effort to call the sensation back." (An unsuccessful experiment.)

Observer M. "Felt current distinctly as little intermittent pokes into tongue. Less difficulty with tongue than usual, *i. e.*, less wiggling. Discovered that what had once seemed like a gap in the current was really filled with minor vibrations which seemed to require an active shift of the attention to get hold of."

Observer G. "Intensity more regular this time; in right electrode I felt the little beatings quite distinctly. Sometimes a cold, metallic taste fused with the electro-cutaneous sensations."

Here again it seems obvious that the observers were able to notice not only any shift of attention to a source of distraction, but also whether or not attention remained maximal during a disappearance of the original sensation. The fact that it did remain constant for at least 2 minutes leads us to the two-fold conclusion (1) that liminal electro-cutaneous sensations under favorable conditions do not fluctuate, and (2) that even if interruptions occur in such sensations owing to external disturbances, attention itself may remain concentrated, merely shifting its focus from the lost sensation to the intruding distraction and back again. It seems reasonable to suppose that the disturbances would not have been sufficient to distract attention if it had not been directed to a single liminal cutaneous stimulus which is very monotonous and almost indifferent as to its affective tone, but to supraliminal stimuli or, better still, to topics of greater diversity and of higher affective coloring. This is borne out by almost daily observations in actual life, such as the intensive reading of an interesting novel, listening to a piece of music, what is generally called absent-mindedness, etc. Here certainly attention remains maximal for a considerable length of time, while there is a rapid change of the mental processes occupying the focus of attention. But fuller evidences of this fact are greatly needed, and must be obtained by a more systematic appeal to introspection with this particular end in view. That our own introspective records are not sufficiently explicit in this regard is due first to the fact that our observers tended rather to justify occurring distractions than to emphasize the periods when attention was absolutely concentrated upon the stimulus; and secondly to the fact that the stimulus set up a complex of sensations which they were eager to analyze and describe. Nevertheless, our final conclusion must be that under favorable circumstances attention focussed upon liminal and supraliminal cutaneous sensations remains approximately constant for at least 2 to 3 minutes, provided that physiological adaptation of the sense-organ and violently intruding distractions can be avoided for this length of time. The normal course of events is that the sensation, once set up-fades out steadily and gradually in consequence of adaptation.

A QUICK METHOD FOR DETERMINING THE INDEX OF CORRELATION.

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The desirability of substituting an accurate numerical index for mere verbal expressions of correlation has been very clearly set forth by Galton, Pearson, Yule, Spearman, Wissler, and other writers.[1] But the most accurate formula, the 'product-moments' formula of Pearson, is attended with arduous labor. We may greatly abridge the numerical work by the use of an adding machine[2] and of appropriate tables, such as Barlow's *Table of Squares*, and Krelle's *Multiplication Tables*. Further, the computation of σ, the standard deviation, may often be reduced by considering it as equal to *m. v.*, or the average deviation, times the constant, 1.2533. Yet, even so, the task is considerable, so that, particularly if one has to determine a number of correlations, it is desirable to use a shorter method for preliminary exploration.

A number of shorter correlation methods have been described.[3] It is the purpose of the present article to describe a simplification of one of these methods that the writer has found very expeditious and serviceable for the determination of an approximate numerical correlation. This method is based upon the use of what is known as Sheppard's formula, which may itself be regarded as a simplification of one of Pearson's auxiliary methods.

For the application of this, as of most formulas, the data of each of the two series to be compared must first be distributed in an orderly array. Suppose, to take a concrete instance, we wish to ascertain the correlation between the accuracy with which 50 boys cancel *e* from a printed slip and the accuracy with which the same 50 boys cancel *q*, *r*, *s* and *t* from a similar slip. The results of each test are first arranged in order, the least accurate boy first and the most accurate last. We can then either determine the average, in which case all the boys that rank below the average are minus and all that rank above are plus, or we can simply take the median value and consider the first 25 boys in each array as minus, and the second 25 as plus, cases. By rapid comparison the following values are next determined:

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[1] See, for instance, F. Galton, *Natural Inheritance;* C. Spearman, The Proof and Measurement of Association Between Two Things, this *Journal*, XV, 1904, 72; C. Wissler, The Correlation of Mental and Physical Tests, *Psych. Rev. Mon. Supp.* No. 16, 1901. The important contributions of K. Pearson, and R. Yule, will be found in the *Proc. Royal Soc.* of London, in the *Phil. Transactions* of the same body, the *Jour. Royal Stat. Soc.*, and, in their more recent applications to biological problems, in the several volumes of the *Biometrika*, 1901 ff.

[2] An inexpensive, but very serviceable device, known as the Gem Adder, is now put on the market by the Automatic Adding Machine Company, Broome St., New York City, at a price of fifteen dollars, and is well worth purchase by any one who contemplates correlation work.

[3] See, for example, the articles by Spearman and Wissler.

a = no. cases that are plus in the 1st and plus in the 2d series.
b = " " " " plus " " " " minus " " " "
c = " " " " minus " " " " plus " " " "
d = " " " " minus " " " " minus " " " "

The index of correlation may now be obtained by reference to one of Pearson's simpler formulas:

$$r = \sin \frac{\pi}{2} \frac{\sqrt{ad} - \sqrt{bc}}{\sqrt{ad} + \sqrt{bc}}$$

Now this formula may be brought into a more convenient form if we replace the sine by the cosine of its complement.

$$r = \cos \left[\frac{\pi}{2} - \frac{\pi}{2} \frac{\sqrt{ad} - \sqrt{bc}}{\sqrt{ad} + \sqrt{bc}} \right]$$

when we can reduce to

$$r = \cos \frac{\sqrt{bc}}{\sqrt{ad} + \sqrt{bc}} \pi.$$

If, now, we further simplify by substituting for the square root of the product of the *b* and *c* cases the percentage of cases with unlike signs (U), and for the square root of the product of the *a* and *d* cases the percentage of cases with like signs (L),[1] we obtain Sheppard's formula:

$$r = \cos \frac{U}{L + U} \pi$$

The results of this formula do not differ appreciably from the foregoing as the value of the fraction is virtually identical.

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[1] That is, virtually, substituting the arithmetical for the geometrical mean.

Now, since L + U must always equal 100, and since $\pi = 180°$, this formula may be written for greater convenience,

$$r = \cos U \; 1.8°$$

Finally, since the values of U must range from 50 to 0 for positive, and from 50 to 100 for inverse correlations, it now becomes possible to prepare a simple table from which the values of *r* for any integer value of U may be read directly, and I have here introduced this Table in the hope that it may prove of interest and assistance.

Correlation Table.

for the formula $r = \cos U \; 1.8°$

If U is greater than 50, first substract it from 100, then prefix the minus sign to the correlation indicated.

U	r	U	r	U	r	U	r	U	r
0	1.000	10	.951	20	.809	30	.587	40	.309
1	.999	11	.941	21	.790	31	.562	41	.279
2	.998	12	.929	22	.770	32	.536	42	.248
3	.995	13	.917	23	.750	33	.509	43	.218
4	.992	14	.904	24	.728	34	.482	44	.187
5	.987	15	.891	25	.707	35	.454	45	.156
6	.982	16	.876	26	.684	36	.426	46	.125
7	.976	17	.860	27	.661	37	.397	47	.094
8	.968	18	.844	28	.637	38	.368	48	.062
9	.960	19	.827	29	.613	39	.338	49	.031

It will be seen, then, that the discovery of the approximate numerical value of the correlation between two series of data is reduced to four simple steps, (1) distribution of the data into two arrays sectioned at the median, (2) counting the cases with unlike signs and (3) dividing this number by the total number of cases, (4) reference to the Table.

The probable error may be calculated from the formula:[1]

$$p.\,e. = \sin \left[0.1686\, \pi\, (1 - r^2) \sqrt{\frac{1}{a} + \frac{1}{b} + \frac{1}{c} + \frac{1}{d}} \right]$$

To illustrate the employment of these methods, in the example cited the following values were obtained: $a = 18$, $b = 11$, $c = 8$, $d = 13$. Hence $U = 38$. By the use of either short formula, $r = +.37$ with p. e. $= .26$. By the use of Pearson's product-moments method we obtain for the same arrays, $r = .47$, with $p.\,e. = .06$, but, by actual timing, after the distributions had been made the first method occupied eight minutes and the second two hours and fifteen minutes, even with the aid of the adding machine and the Tables previously mentioned.

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[1] The value of $(1\text{-}r^2)$ for all values of *r* may be obtained directly from a Table published by Yule, *Jour. R. S. Soc.*, X., 1897, 852-3.

The short method cannot, of course, be recommended for the final determination of important correlations, because the probable error is large, particularly with relatively few cases and a low value of r, but it is very serviceable for the preliminary examination of such data, and may give results of value when the scale divisions are fairly fine, the data symmetrically distributed, the number of cases not too small, and the correlation large, say above 0.50.

SOME EXPERIMENTS ON THE ASSOCIATIVE POWER OF SMELLS.

By E. M. BOLGER and E. B. TITCHENER.

This brief communication may be regarded as a foot-note to the study published under the same title in 1905 by the Misses A. Heywood and H. A. Vortriede.[1] The object of that study was to make a laboratory test of the observation, not uncommon in everyday life, that odors possess a remarkable power to revive past experiences. The method followed was, in general, that used by Professor Calkins in her work on association. Combined series of odors and pictures, and of odors and colored squares, were presented to the observer, and the series of odors repeated later in a different order; the percentages of correct associations were then noted. The results were checked by those of series in which the odors were replaced by nonsense syllables (auditory stimulus). The authors conclude that "the odors have no superiority in suggestive or associative power over the nonsense syllables."

It seemed to us that it was worth while to repeat these observations. We hoped, on the one hand, to go a little more deeply into the qualitative analysis of association, and on the other to get suggestions for an improved method of attacking the problem. Both efforts were unsuccessful. The Calkins method is, without any doubt, too crude for analytical work, and the change that we ourselves introduced led only to negative results.

We first repeated the Vassar experiments, with two minor variations. The time of exposure was reduced from 15 to 5 sec., on the ground that a time of 15 sec. might give opportunity for wandering of the attention. Secondly, while we employed the 6-term series with one observer, we raised the number of terms to 12 for two other observers: it was possible, we thought, that the odors might show their superiority under the more complicated conditions. The results need not be given in detail. They resembled those obtained in the Vassar laboratory, except that the odors showed at a still greater disadvantage. It was clear that, under the conditions of these experiments, an exposure time of 5 sec. does not allow an odor to impress itself upon the attention.

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[1] Minor Studies from the Psychological Laboratory of Vassar College, communicated by M. F. Washburn, i. This *Journal*, XVI, 1905, 527; XVII, 1906, 148.

We then had recourse to a change of method. The odors that in everyday life prove effective for recall are diffused odors; the "smell of box" that "recalls a garden frequented in childhood" was probably dominant over a wide area of the garden, and was thus associated to a varied and complex visual situation. We tried to reproduce this condition by suffusing our pictures with odors, instead of presenting the odors separately in phials. A careful selection was made of 50 picture postcards. Envelopes, filled with cotton wool, were pasted to the backs of the cards; and the wool was saturated with the odorous stimulus before the experimental series began. The odors were distributed, as evenly as possible, over Zwaardemaker's nine classes. Every care was taken to keep the stimuli from mixing or becoming contaminated during the course of the tests. The cards were arranged in 5 series of 10 members, and the experiments were made with two observers, both of whom had taken part in the previous work. The time of exposure was 10 sec. for the one and 15 sec. for the other observer.

The stimulus in the combined series was now a scented picture; in the test series, the scent alone. Nevertheless, although the conditions were by so much nearer the conditions of everyday life, the outcome was still negative. The odors were no more effective than nonsense syllables (auditory stimulus), and were distinctly less effective than simple geometrical figures drawn with ink in a corner of the pictures. The result was the same for both observers, and therefore for both exposure times.

The next step in method would be, perhaps, to make up combined series of a mixed sort,—one or two of the pictures being scented, and the rest paired with nonsense syllables or colors or geometrical forms. We were, however, compelled to break off the investigation at this point.

GENERAL PRACTICE EFFECT OF SPECIAL EXERCISE.

By J. E. Coover and Frank Angell.

Somewhere in the course of a lecture on the Relation of Natural Science to General Science, Helmholtz speaks of the advantages possessed by certain studies as "means for training the intellect, inasmuch as they tax equally all the intellectual powers," and this belief in the general disciplinary value of certain studies,—the greater or less efficacy of certain kinds of intellectual work as mental gymnastic—probably forms the *raison d'être* for education in the minds, not only of most laymen, but perhaps of most teachers of the present time, though what may be the factors entering into this disciplinary process is a question which usually receives a broad or vague answer. But of late years, under the title of formal discipline, the tenet has lost considerably in dogmatism and gained somewhat in definition, so that it has become a subject for experimental investigation. The present article is a contribution to this aspect of the subject, *i. e.*, whether it can be shown by direct experiment that the improvement acquired by practice in one kind of mental activity is induced in some other kind, more or less remote.

The greater part of the literature of the subject has been too frequently thrashed out to need further mention here. Of the less commonly quoted, articles which bear directly on the question may be cited, 1st, Urbantschisch's investigation in *Pflü. Arch.* Bd. 42, in which he showed that the influence of a sound stimulus is to lower the limens for light, smell, taste and dermal sensations; 2nd, Epstein's work, published in *Zeit. f. Biol.* Bd. 33, indicating that a sound stimulus heightens visual acuteness and sensitiveness to color, and 3rd, Vogt's research on Distractibility and Habituation in Kraepelin's *Psy. Arb.* Bd. 3, demonstrating that habituation to a distraction acquired in one exercise may be carried over to other exercises. There remain, however, two investigations, which, being of more recent date, have passed less frequently through the critical mill and therefore call for more detailed examination; Ebert and Meumann's extended research on certain fundamental questions connected with memory practice in *Arch. f. d. ges. Psy.*, Bd. 4, and Thorndike and Woodworth's work on the "Influence of Improvement in One Mental Function upon the

Efficiency of other Functions," in Vol. 8 of the *Psy. Review*. The treatise of Ebert and Meumann is an investigation of the induced effects of one kind of memory training on other kinds of memory carried out according to the "test" method. More especially, "a cross section" of memory was taken, determining the efficiency of the several memories for meaningless syllables, letters and numbers, one-syllabled substantives, foreign words (Italian), stanzas of poetry, visual signs, and prose. The reagents were then trained on meaningless syllables, when another "cross section" of memory was taken, to test both immediate and permanent retention. The tests indicated that the special training had increased the efficiency of all the several kinds of memory and that for the kinds of memory most closely allied to the test processes, the increase in efficiency was greatest. Thus, the authors state that the training increased the efficiency of memory for retaining philosophic prose 70%, and the memory for visual signs—a process certainly very unlike the training process with meaningless syllables—showed an increase in efficiency of over 55%, (*Arch. f. d. ges. Psy.*, 4: 182). In fact the increase in memorizing ability for all kinds of memories was so great that the reagents came to designate the training process as "memory cure." It is to be regretted that the authors did not carry on a "control" experiment along with their tests to ascertain the training effect of the tests themselves and to throw additional light on the changes taking place in the training intervals. Nevertheless, the investigation indicates pretty clearly that the training of a special memory increases the power of memory in general. The answer to the question how the learning of meaningless syllables increases the efficiency of all other kinds of memory is answered by the authors with an explanation involving a sympathetic practice effect of allied memory functions through a hypothetical psychophysical action. But as G. E. Müller has pointed out in his review of the work (*Zeit. f. Psy.*, 39: 113), the known causes of improvement, regarded by the authors as merely auxiliary, such as increase of power of concentrating attention, increase in effort to perfect a memory, decrease in feelings of discomfort and tediousness, improvement of technique of learning—all of these should be logically regarded as "cause" rather than a mysteriously hypothetical psychological action.

The investigation of Thorndike and Woodworth was also carried out according to the test method, its object being to ascertain the result of training with weights, areas, and lengths on the ability to estimate other weights, areas and lengths respectively, that differed from the training series either in size or shape or both. In addition they tested the effect of training, in the marking out of certain letters or misspelled words on a printed page. One misses, however, those qualities which make Ebert and Meumann's investigation of first rate importance, particularly the careful elaboration of the plan of work, the actual working out of the method in the form of detailed introspections, and the searching and thorough analysis of results. The general conclusions from these experiments are given in Thorndike's "Educational Psychology," p. 91, and are to the effect that "the improvement in any single mental function need not improve the ability in functions commonly called by the same name," and further that "the practice effect seems to make it likely that spread of practice occurs only where identical elements are concerned." It does not appear that the conclusions of the authors of the article are wholly borne out by the figures, the test on estimating the size of surfaces, *e. g.*, showing greater improvement for figures which were unlike the training series in size and shape than for those nearest in either size or shape. The writers state that their experiments were rough and designed to show merely general tendencies, but it is not clear that the figures show anything beyond great individual differences. Had the figures shown a loss of efficiency in executing the tests, the question of transference of function would have still been open. Rough experiments unaccompanied with introspection and so not susceptible of careful analysis are of very little value in work of this kind. The marking out of words is a complicated discriminative reaction, which is not only performed in very different ways by different reagents, but in different ways by the same reagent in one sitting. How different these ways may be will be shown later in an analytical paper by Mr. G. Snow-Gibbs on "Tests in Applied Psychology." On the other hand introspective data gathered by one of the present writers in repeating these experiments (J. E. C.) seem to indicate clearly that the improvement in after tests in marking out letters is not due to the functioning of "identical motor elements," viz., eye movements for example, but to a reduction of the recognition of a word as containing certain letters to its essential process. The process had been relieved of the unnecessary and retarding accompaniments (kinæsthetic and acoustic imagery) noted in the first tests and in the early stages of training. In other words the improvement was due to the general function known as habituation.

At any rate it seemed advisable to attack the problem in such a way that the factor of identical motor elements could play no important part, so that if improvement in efficiency was shown in the test, it could only be attributed to some such general factor as habituation. Accordingly tests were made on sensible discrimination in different modalities of sense and on discriminative reaction with choice in which the reaction motions common to the test and training series were a negligible quantity. In addition, control experiments were carried along to indicate the effects of test series without the training.

I. TEST IN LIGHT DISCRIMINATION WITH TRAINING IN SOUND.

Four reagents were trained in discrimination of intensities of *sound* for 17 days during an interval of 57 days. Each reagent made 40 judgments in each day's training.

Before and after training the reagents were tested in the discrimination of shades of gray, each test consisting of three series, each containing 35 judgments, delivered on 3 separate days.

In the training on sound the stimuli were given with a sound pendulum. The method was that of constant difference, procedure being without knowledge. There were about 10 values for D in each series. The judgments were made in four categories, viz., louder, softer, like, and doubtful. In one pair of series each day, introspections were noted down by the reagent after each judgment; in the remainder the noting of introspections was reserved until after each series was finished.

Discriminative ability at the beginning and end of the training was calculated in per cents of Right cases in the first ten judgments and the last ten judgments made upon six each of the values of D, that is, upon most of the judgments of the first and last two days. No judgments were included where D=0, and "like" judgments for other values of D were regarded as undecided.[1]

The tests on brightness were made with a Marbe color mixer so mounted as to run noiselessly. The experiments took place in a dark room with the light coming from behind the reagents. All the apparatus, including the disk when not exposed, was draped in black. Norm and variable were exposed for two seconds with an interval of four seconds. The attention signal came two seconds before the exposure of the norm. The method was also that of constant differences, without knowledge and the judgments were given in the categories of "lighter," "darker," "like," and "undecided." Introspections were noted by reagents after each series of seven judgments. The order of variables in the test before training was repeated in the after-training test, but in both cases was believed by the reagents to be determined by chance. In order to estimate the possible practice effects of the preliminary test and the interval between that and the after-test, a control experiment was carried along. In this three reagents were given tests in brightness discrimination under conditions identical with those obtaining with the regular reagents, except that 2 instead of 3 days were taken as a basis of comparison. At the end of the interval used by the test reagents for training, *i. e.*, 46 days, the control regents took the second test.

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[1] F. Angell: Discrimination of Shades of Gray, etc. *Phil. Stud.*, XIX, S. 20.

Table I gives a summary of the numerical results for both test and control reagents. From this it will be seen that all the test reagents with one exception show a gain in Right and loss in Undecided judgments after training, and that all the control reagents show a loss in discriminative ability after the interval of rest which was occupied by the test reagents in training. The per cent. of gain for the 4 test reagents was 4, 4, 6, 6, 0, 0, and 27, 5, making an average of 9, 1. The case of the one test reagent who showed no improvement in the after test would seem to be a fair example of the exception which proves the rule, for while the other reagents in the training on sound all showed improvement which amounted to 7, 5, and 15 per cent. respectively, this particular reagent showed no improvement in the judgments of the last two days as compared with the first two, and in fact had fallen off to the extent of three per cent.

The introspections indicate that the discrimination processes were accompanied by much imagery from other domains of sense which in some cases determined the judgment. This imagery was in great part kinesthetic and visual. In addition the sounds seemed to have certain spatial or temporal attributes which at times influenced the judgments. "Variable had a long drawn out sound;" "Variable is a broader sound, that is to say, widely spread;" "Variable comes from a farther place;" "Variable is a small sound." Again bodily impressions may have entered into the act of comparison such as "resonance" or "ringing" in the ears or seemingly deeper seated sensation in the head. "Discriminate effects in the head rather than external sounds." One reagent seemed to compare the intensities of bodily reactions to the sound stimuli themselves or to imagery called up by the stimuli, *e. g.*, the "flash of a bicycle lamp."

Besides these various reproductive factors which accompany the essential sound-discrimination process, there are disturbing factors of a general nature, such as strong expectation for a loud or weak sound, and the varying intensity of the state of attention. If a loud sound is strongly expected, a weak one may seem weaker; if a weak one is expected a loud one may seem louder. If the intensity of the state of attention is sought to be kept at a maximum, it will vary greatly, a result

TABLE I.

Comparison of Test and Training Judgments for Test Reagents and of Test Judgments for Control Reagents.

Test Reagents.

A.	Right Judgments.	Aw	Na	Ya	Cr	Total.
Tests.	No. R. Before training. " " After "	60 64	46 52	47 47	46 68	119 231
	Diff.	+4	+6	0	+22	+32
Training in sound.	No. R. 1st 2 days. " " last 2 days.	22 31	22 25	26 24	27 34	97 114
	Difference.	+9	+3	—2	+7	+17
B.	Undecided Judgments.					
Tests.	No. U. Before training. " " After "	16 13	38 19	22 28	27 6	103 66
	Difference.	—3	—19	+6	—21	—37
Training in sound.	No. U. 1st 2 days. " " last 2 days.	18 9	23 16	16 20	16 8	73 53
	Difference.	—9	—7	+4	—8	—20

Control Reagents.

C.	R. and U. Judgments.	Rl	An	Wr		Total.
Tests before and after interval without training,	No. R. Before interval. " " After "	41 38	31 30	46 43		118 111
	Difference.	—3	—1	—3		—7
	No. U. Before interval. " " After "	10 13	16 15	2 7		28 35
	Difference.	+3	—1	+5		+7

due both to its own rhythm and to the varying subjective conditions upon which it depends.

Improvement seems to consist of divesting the essential process of the unessential factors, freeing judgments from illusions, to which the unnecessary and often fantastic imagery gives rise, and of obtaining a uniform state of attention which is less than a maximum: "Judgment does not require strained attention. All are quite certain or satisfactory. Don't see what the process is now—seems automatic" (Cr. May 12, IV). And uniformity of direction of attention may also result: "Am able to abstract from visual image of the apparatus entirely, and yet refer the sounds to external stimuli. This seems to take the least effort and is more satisfactory" (Cr. May 17, IV). Many of the introspections of the various reagents, near the end of training, were "No imagery."

Our conclusion from the experiment, therefore, is that efficiency of sensible discrimination acquired by training with sound stimuli has been transferred to the efficiency of discriminating brightness stimuli, and that the factors in this transfer are due in great part to habituation and to a more economic adaptation of attention, *i. e.*, are general rather than special in character.

II. Reaction with Discrimination and Choice.

In the experiments with discrimination and choice identical motor elements were eliminated by employing different kinds of stimuli and different forms of reaction, although in both cases the sense of sight received the stimuli, and reactions were made by movements of the hands.

The object of the experiment was to determine the effects of practice in one form of activity upon efficiency in another. This influence could show itself either in lowering reaction time in the latter, or making it more regular, or both,—which would be apparent in a practice curve of the tested ability.

The training was accomplished by about 15 exercises in sorting cards. Four reagents took the training which was scattered through a space of about 40 days. In this time about 4,000 cards were distributed by each reagent (Cl 4,200, Al 3,800, Cr 5,200, Bs 4,000).

Before the training in card-sorting the reagents were tested for five days on "typewriter reactions," aggregating about 3,000 reactions in each case (Cl 2,900, Al 2,900, Cr 2,700, Bs 3,100), and after training in card-sorting there came the after test for three days, giving about 1,800 reactions (Cl, Al, Cr, 1,800, Bs 1,700).

For the training a Jastrow card-sorting cabinet was used with 6 compartments; the cards were made of smooth, buff-colored Bristol-board (77x52 millimeters). In the centre of each card a colored rectangle was painted in water color (12x 52 mm.), six colors being used—red, blue, black, and brown in a rather deep shade, and yellow and green in tints. The cabinet stood at a convenient height, and was entirely covered with black cloth.

In card-sorting the reagent stood at the cabinet and held in his left hand a pack of 50 cards, from the top of which he would grasp a card, turn it up sufficiently to see its color on the under surface and drop it into its appropriate compartment. In about the middle of the training the color labels were removed from the compartments.

The cards were arranged in packs of 50, according to 12 different orders in which each color appeared about as often as every other, each preceding and succeeding the other about equally often; no color recurred with less than two intervening colors.

The assignment of colors to the compartments was so made that the spatial relations of the latter would not correspond with the complementary or spectral relations of the former.

For the "typewriter-reaction" a Blickensderfer typewriter was used, fitted up with a screen through which but one letter could appear at a time. Series of letters were printed with the typewriter and cut into strips which could be clipped to the "scale-bar" and moved behind the screen by the "carriage." The spacing of the letters in the series and of the typewriter "action" being the same, the strip could be so adjusted that every time a key was struck, a new letter appeared through the screen.

The various series were made up of four letters in such a way that all the letters appeared about the same number of times, and each letter preceded and succeeded every other letter and itself about equally often.

In the "typewriter-reaction" the reagent sat with his hands in position over the lower bank of keys on either side of the middle, and reacted to the letters as they appeared through the screen—*a* and *t* on the left and *e* and *n* on the right.

The time of reaction to each letter was recorded in another room upon a kymograph. The typewriter itself, of course, making records of the reactions, which could be inspected for accuracy.

Control Experiment. In order to determine more definitely the possible effects of the training in card sorting in the "typewriter-reactions," three reagents were trained in the "typewriter reaction" three days before and two days after an interval of 45 days during which no training was taken. In both the test and control experiment, efficiency was estimated by the time involved and in number of errors in each 100 reactions.

Table II gives the daily average time and error for 100 reactions on the typewriter for the test reagents, both before (A) and after (B) training in card-sorting, and Table III gives like data for the control reagents, both before (A) and after (B) an interval without practice.

The quantitative results as laid down in those two summaries seem to be ambiguous: the reagents trained on the card-sorting show indeed an increase in efficiency in the after-test on the

TABLE II.

Test Reagents. Daily average of time (in sec.) and errors for 100 reactions on Typewriter.

Cl		Al		Cr		Bs	
A. Before Training.							
sec.	errors.	sec.	errors.	sec.	errors.	sec.	errors.
71	2	94	0	73	3	99.5	5
76	0.9	93	4	76	3	87.3	3
73.5	1.1	80	3	71.2	3.5	84	4.5
67.4	1	73.1	6.3	69.1	3	80.1	5.3
69	1	72.2	4.5	69.1	5	77.9	4.3
63.3	0.8	72.7	7	67	4		
B. After Training.							
63.1	1	65.2	8	66.4	4.5	70.7	8
61.3	1	62.9	11	62.1	4.5	69	3.3
60.4	1	61	13	61.7	5.4	66.3	6

TABLE III.

Control Reagents. Daily average of time (in sec.) and errors for 100 reactions on Typewriter.

sec.	errors.	sec.	errors.	sec.	errors.
A. Before Interval.					
90.7	5	141.8	4	87	1
74.1	7.5	116.5	1.3		
74	6.3	96.1	1.5		
B. After Interval.					
70.7	4.5	90.5	1.3	80.5	1
66.2	7	86.2	1.3		

typewriter, but most of them show an increase in errors. Moreover the control reagents also show an increase in efficiency after their incubation period. In both cases, however, an increase in speed was to be expected, for the common belief in beneficial effects of incubation periods on bodily activity has been amply confirmed by numerous investigations on practice and fatigue. The question of the effects of the training in card-sorting must, therefore, find its answer in an examination of the errors and of the introspective evidence. The obvious explanation of the increase in speed accompanying the decrease in accuracy is that as the reagents increased in manual dexterity they became more careless in co-ordinating the visual impressions with the reactions; in short, that accuracy was sacrificed to speed. But a study of the errors shows that the obvious explanation is not correct, for taking from each of the eight test days the series of reactions showing the maximum and minimum number of reactions respectively, we get

TABLE IV.

Table of Relation of Reaction Times to Max. and Min. Number of Errors.

Cl	with	0,06	errors	averages	66,4	sec.	for	100	reactions
	"	2,3	"	"	67,9	"	"	"	"
Al	"	4,7	"	"	72,0	"	"	"	"
	"	9,9	"	"	73,0	"	"	"	"
Cr	"	1,4	"	"	65,6	"	"	"	"
	"	5,9	"	"	68,4	"	"	"	"
Bs	"	2,3	"	"	77,8	"	"	"	"
	"	7,9	"	"	81,6	"	"	"	"

The figures from which these averages are taken occur irregularly throughout the several series of the eight days, so that it cannot be urged that the correlation of a smaller number of errors with greater speed is due to each day's practice effects. As a matter of fact in the majority of the eight cases, the maximum number of errors occurred in the later series of the day. The introspections show the reasons for this inverse relation existing between speed and error tendencies. Cr notes "Made mistake and was bothered thereby;" "Errors result in confusion and pauses." Al remarks, "The large number of mistakes impedes the rapidity as one is troubled by them;" Bs says "Mistakes were noticed and caused confusion at the time." Cl says "The time I spent in thinking of a mistake caused a delay." From both tables and introspections we see that the increase of errors in the after test can be due only in a small part to carelessness accompanying greater skill in manipulation, and the question then arises, to what is it due?

There were probably at least five special causes for the errors: (1) Lack of co-ordination between the letter and its proper reaction, (2) Anticipation of a letter, in which case the reaction took place before the letter was cognized, (3) False recognition of the letter, (4) Reaction incited by rhythm without cognition of letter, and (5) Misplacement of the fingers on the keys. The fourth cause is probably the chief factor in the increase of errors in the second training.

As the reagents acquired more and more skill on the typewriter, a strong tendency developed towards rhythmic series reactions which resulted not in false or mistaken reactions, but in mechanical reactions; that is, the rhythmic tendency was strong enough to overcome the voluntary effort towards a discriminative reaction. The effect of this power was to produce the confusion referred to above and in general to lengthen the total time for the series. A general source of error for the after-tests was the fact that they came in the closing days of the semester when the burden of the examination period fell heavily on instructors and students alike. The introspections give altogether too full evidence of the condition of general fatigue with which the reagents entered on the tests after training.

There remains to be discussed one effect of the training which cannot be shown by tables, and that is the effect of training on ease of accomplishment. The figures for the after-test on the typewriter show that the test reagents made very little gain in speed for the three days of the test itself. Indeed the skilled reagents Cl and Cr had arrived at their maximum efficiency one or two practice days before the training period, and if the card-sorting had favorably affected the speed of the typewriting it could not be shown by numerical results. But the introspections show a very unexpected increase in the ease of accomplishment following on the card-sorting.

On the first day of the second test Cl remarks, "Sight of letter produced the reaction movement without my thinking of my fingers or of the sight of the keys." Al remarked, "No headache, no nausea as before card-sorting," "Much easier than at first;" "General background of feeling is probably not unlike that of the card-sorting test, but I did not think of the card-sorting test during the trial." Cr said: "Process was surprisingly automatic and was accompanied with ease," "Seems more automatic than ever before, and even more so than the card-sorting. I do not pay the slightest attention to the fingers on the keyboard when the process is going best. . . . It appears that the old associations have not only not been interfered with by forming new ones in card-sorting, but that they have become firmer and action upon them more ready and automatic than it was before or than it was in card-sorting" (April 27). On the last day of the typewriter training before card-sorting began, Bs remarked, "Pauses between letters caused by having to think which finger I should put down," and on the first day of training after card-sorting, "Seemed more natural than I thought it would," and the next day, "Seemed more natural to react to-day, demands less attention, tendency to become automatic."

The introspections of the Control reagents, on the other hand, none of whom was familiar with typewriting, show that while some ease and facility were experienced in the period before training, the after-test seemed unexpectedly "difficult" and "unhandy;" Mn stated in the after-test that "Reacting seemed difficult," "Seemed to have to stop to think which finger was to react to the different letters." Ge in the period before training said, "The reactions are becoming more automatic;" and in the after-test that he "looked at a letter not knowing what to do," and that "combinations of letters here were particularly unhandy." Feelings of discomfort and difficulty following on the interval without practice of any kind emphasize the conclusions of the preceding paragraph, viz.: That the training in card-sorting is the cause of the increased ease and facility experienced by the regular reagents in the second trial in typewriter-reaction.

Conclusion. We may conclude, therefore, from the results of this experiment, that training the activity of Reaction with Discrimination and Choice by sorting cards into compartments has increased the facility of a like activity in both speed and regularity in "typewriter-reaction" (a) noticeably, in two cases, after the latter had become automatic, and (b) markedly in two others, in the course of practice.

The cause of transferred facility could not have been identical motor elements. In the "typewriter-reaction" the eyes rested sharply fixed upon one spot upon the screen, while in the card-sorting reaction the eyes moved rapidly about over the compartments in the cabinet, merely glancing at the colors. The differences in manipulation between tapping keys with forefingers and sorting cards are, of course, patent. To what, then, is this improvement due?

According to the introspections of the regular reagents on their card-sorting training, the process of reaction is variable. At the beginning of training they matched the color of the cards with the labels on the compartments; then to increase speed a system of mnemonics is employed, designed to form associations in the mind between a compartment and its color; this system then goes through a process of mutation,—becoming abbreviated, changed in part, supplemented, or is superseded by another; finally, through repetition, reactions to particular compartments become co-ordinated with their respective colors and are made directly—free from any "system" except in rare cases. Synchronously with the growth of these co-ordinations adventitious processes, such as pronouncing the name of the color when cognized, movements of the whole body and useless movements of the hand, decrease to a minimum.

As regards the "typewriter-reactions," the introspections of both regular and control reagents show that the processes of reaction go through like stages, except that the mnemonic systems arise sooner and change oftener. No two systems are alike. But all finally give way to the direct reaction which has been co-ordinated with its letter. Again, adventitious processes, such as pronouncing letters upon cognizing them, visualizing keys, fingers, or their order, likewise decrease to a minimum.

Different as the typewriting and card-sorting reactions are in details the course of experimentation developed a general condition common to both, *i. e.*, *the habit of stripping the essential process of unnecessary and complicating accessories.*

Introspections further show that in any one series several systems of mnemonics may be operative; there may be also some direct reactions due to the co-ordination of stimulus and reaction; in one part of the series one stimulus causes difficulty and elsewhere another stimulus, while some stimuli seem harder than others throughout the whole series. Improvement here seems to consist in resolving the reaction process to a single type (except in so far as reactions become direct), and in attending more closely to difficult stimuli until their reactions become as ready as those to the other stimuli.

Again, introspections and records show that, even after a mnemonic system has been successfully applied and has served to bring stimulus and reaction to a fair degree of co-ordination, lapses of attention occur during which the "mind is a blank," and the drum records abnormally long reactions. Improvement here consists in keeping attention upon the matter in hand so constantly that the irrelevant stimuli are unnoticed.

We find, therefore, the causes of the transference of facility to be: (a) the formation of a habit of reacting directly to a stimulus without useless kinesthetic, acoustic, and motor accompaniments of recognition, which results in (b) an equitable distribution of attention to the various possible reactions so as to be about equally prepared for all; and (c) the consequent power of concentrating the attention throughout the whole series without distraction.

GUSTATORY AUDITION; A HITHERTO UNDESCRIBED VARIETY OF SYNÆSTHESIA.

By ARTHUR H. PIERCE, Smith College.

During the lifetime of this *Journal* there have appeared on its pages a goodly number of cases presenting one or another of the various forms of synæsthesia. While such accounts do not yield any facts of profound import for psychology, they do yet have their distinct value in impressing us with the myriadfold divergencies of human nature, and it is just for this purpose of contributing to the 'varieties' of mental experience that the following case is presented. As far as I can ascertain, its peculiar variety is new in the annals of synæsthesia. Its singularity consists, I may say at once, in the experiencing of gustatory qualities following upon the hearing of words spoken by the human voice, or of musical and other non-vocal sounds.

As it stands, however, the statement just made is by no means a correct description of the case. Perhaps the best way of putting the matter would be to say that the word 'gustatory' must here be taken in a very wide sense as referring to all the possible experiences that the sense organs of the mouth can give. To speak with full accuracy one must say that a 'mouth-experience' of one sort or another is the accompaniment of certain auditory impressions. These 'mouth-experiences' include the four taste qualities, the cutaneous qualities of pressure and temperature, and such tactual-motor qualities as find their expression in designations of texture, consistency, and the like. Now it is only because we have no single adjective by which we commonly describe this variety of possible mouth-experiences that the word 'gustatory' is here used with the latitude indicated. Its use enables us to avoid an uncouth terminology, and with the explanation given no obscurities are likely to arise. One further fact should be added. Olfactory qualities are entirely absent from the synæsthesia, and we are concerned, therefore, with pure 'mouth-experiences' without any of those numerous fusions which popularly pass for 'tastes.'

Hitherto we have had descriptions, plentiful enough along certain lines, of colored sounds, odors, tastes and temperatures, and of total pain, pressure, and temperature. But I am not aware that we possess any records recounting the type that I have just indicated.

I.

The subject of the case in question is a young woman, now about to graduate from college, who, as far as she can tell, has always had these experiences, the fact that they were peculiar to herself first coming to notice about four years ago. Since that time their number and definiteness have somewhat increased, owing to the numerous questions put to her by her companions as to the 'taste' of this or that name or word. But though the experiences have increased in number, I am convinced that they have not lost their original naïve character, as the facts to be cited will, I hope, show. The case has been under my observation for somewhat over two years.

Two sense defects of the subject should be noted. First, there is a slight deafness, or slowness of hearing, which becomes more pronounced at some times than at others. Secondly the subject is anosmic. No rigid tests have ever been made to determine whether the sense of smell is entirely wanting, but medical examination directed to the sense of hearing revealed a thickened mucous membrane of diminished sensitiveness, and the subject herself believes that her anosmia is complete. Coffee burning upon a stove is not noticed, though she may be close by. Camphor placed in the nostrils gives only a stinging sensation. Ammonia can be sniffed without discomfort. And, as with all anosmics, foods are discriminated on the basis of the pure taste qualities, or by the characters of texture, astringency, and so on, which any food-complex may possess. These sensory defects are worth noting for their possible significance in connection with the theory of this special case of synæsthesia.

The following list of the 'gustatory' equivalents of spoken words was in part compiled at my request and in part written down by myself in the course of various interviews with the subject. In the latter case the words were spoken by me, and almost immediately the equivalent was given in concise, discriminating terms. Only occasionally was there some hesitation and a groping after the name of the precise food which descriptively satisfied the subjective experience. Because of the unusualness of the case, it has seemed best to transcribe in its entirety the list of 150 words that is before me in my notes.[1] The only change made is the arrangement of the words in their alphabetical order.

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[1] Some further equivalents may be found in Table V.

TABLE I.

Gustatory Equivalents of Words Heard. (English.)

Ah!	Something soft.
Albert,	Not definite.
Alice,	Spanish Cream, with sprinkling of sugar.
Alfred,	Corn-bread in milk.
amethyst,	Bitterness.
Amy,	Ketchup (especially vivid).
Anne or Annie,	Apple sauce stiff with sugar.
Austrian,	White frosting on cake.
Arthur,	Small particles of delicate meat, probably lamb.
Bailey,	Marshmallows.
beast,	(see 'ease').
Belfast,	Juicy beefsteak.
Ben,	Like biting something, as if taking celluloid between the teeth.
Benjamin,	Spice cake (dark).
Bess,	Crumbs of butter. Cool.
Blanchard,	Dried catnip.
box,	Nothing definite.
boy,	Gum drops.
Browning,	Rather rich. Inclines to bitter. Might be over-done gingerbread.
Buddha,	A taste of its own, vivid but indescribable.
Cæsar,	Dry meat.
Carrie,	Onions or asparagus cooked in milk.
cause,	Hot, soft corn-bread.
Charles,	Buttered toast, not dried through.
Clara,	Lobster claws.
cox,	Sensation of irritation in throat.
crease,	Baked sweet potato with much butter.
dice,	Cool, salty.
discrete,	Warm new cake.
distinct,	Preserved pears.
Dolly,	Molasses candy, moist, fluid, taffy-like.
Dora,	Hash.
Dorothy,	'Velvet' molasses candy.
doubt,	Raw apples.
Dwight,	Beefsteak, brown, a little burnt. Or, spruce gum.
ease,	Not clear at first. Then, dry meat, when spoken quickly and sharply. If spoken more slowly, softer, pliable, creaky, like puffed rice.
Edith,	Potato soup.
Edgar,	Boiled eggs, with suggestion of the shell.
Edna,	Boiled eggs.
Edward,	Soft eggs.
Eleanor,	Chicken 'stuffing.'
Eliza,	Piece of twine in mouth.
Elizabeth,	Shreds of well-done roast beef.
Elsie,	Corn-bread in milk.
Emma,	Pie crust.
Eulalia,	Eu=olives. lalia=cooked fruit or jelly.
Eunice,	Intensely sour. Draws the jaws so that there is decided pain.
Eve,	Meat of nuts.
Ethel,	Sensation of putting thimble on tip of tongue.
Evelyn,	Fresh catnip.

fancy,	Cold Boston Baked Beans.
fast,	(see 'Belfast').
feast,	(see 'ease').
Fillebrown,	A serial experience. Water, then, quickly, gingerbread.
Florence,	Maple sugar.
found,	Lamb.
Francis,	Baked Beans.
French,	Charlotte Russe.
George,	Something thick, like a charcoal tablet.
Gladys,	'Velvet' molasses candy.
grin,	French toast, or fried bread.
Hall,	Like rubber end of pencil pressed against tip of tongue.
Hannah,	Dry, nut-like.
Harriet,	Cooked cherries, inclined to bitterness.
Harris,	Nothing.
Harry,	Nothing definite.
Hawthorne,	Agreeable, little sweet, little bitter, smooth, elusive.
Helen,	Blanc mange.
Helene,	Raw oat meal.
he,	Dry meat.
Henry,	Celery.
Hi!	Something cold.
hope,	Celery.
ice,	Like itself.
idle,	Thin, salty broth (lamb?).
Inez,	Boiled cabbage (or greens) with vinegar.
intelligence,	Raw sliced tomato.
interest,	Stewed tomato.
Irving,	Rich sort of word. Cottage pudding with thick, sweet sauce.
Italy,	Very small white pickled onions.
James,	Dry, brittle.
Jessie,	Cool, round rolling pieces, possibly nuts.
John,	Crust of soft, moist gingerbread.
Josephine,	Oranges.
Joy Joyce	Rich cream candy.
Judith,	Shreds of very salt boiled ham.
Katherine,	Tea grounds, or spices.
Kitty,	Soft inside part of a baked apple.
Lena,	(see 'Helene').
Lida,	Mutton tallow.
light,	Soft juicy lobe of a grape, when spoken quickly. Chicken gravy, when spoken slowly.
Lillian (=Lilly),	Sweet apple jelly.
Lola,	Stewed prunes.
loud,	Boiled new potato.
Louise,	Bananas.
Luke,	Not definite.
Lulu,	Floating islands.
Mabelle,	Maple sugar.
Margaret,	Lima beans.
Maria,	Dry browned part of custard.
Marion,	Meat of walnuts.
Marjorie,	Rich cream candy.
marry,	Raisins.
Mary,	Stewed blackberries.

Maud,	Yolk of eggs in salad.
men,	Hash.
Miriam,	Cool, pleasant.
moccasin,	Chicken.
Molly,	Sweet custard.
Nancy,	(see 'fancy').
Newcomb,	Ginger cookies.
noise,	Cold camphor-ice.
Oh!	Sweet.
Oo!	The dip of milk toast.
ox,	Nothing definite.
parlor,	Honey on bread. (Especially vivid.)
Patrick,	Nothing definite.
Phœbe,	Small pieces. Walnuts broken up.
Pierce,	Crisp, fatty part of fried bacon.
Polly,	(see 'Molly').
Rachel,	Ginger cookies.
Ralph,	Moist, cool. Raw cucumbers. (Vivid.)
rejoice, Royce,	Rich cream candy.
rice,	Like itself.
Rosalie,	Sweet, spicy, as if fumes were passing up through the nostrils.
Rosamond,	Almonds.
Ruth,	White grapes.
Sampson,	Potato salad. (Vivid.)
Sarah,	Cold metal between the lips.
Savage,	Raw apples.
Scott,	Skin of a baked apple.
Silas,	'Si' is indefinite. 'las' is candy.
Stella,	Celery.
story,	Stewed cranberries.
Susan,	Milk toast.
Tabby,	Bananas.
Tappan,	Nothing.
town,	Tongue.
tox,	(see 'cox').
tress,	(see 'Jessie').
Union.	Olives.
Uriah.	(see 'Maria').
Vivian,	Moistened bread.
who,	Thick, salty cream.
William,	Cool, clear water.
women,	Hash.
Zechariah,	'Zech' is something hard.
Zedadiah,	'diah' is something stiff, then yielding.

It should be clearly understood that the equivalents above given are meant in each instance to refer to the fact that the subject feels as if she were actually having in the mouth the described substance or some substance possessing the quality indicated.

An examination of the list will reveal the fact that all the possible qualities of gustatory, cutaneous, and tactual-motor experiences are represented. Very rarely, if ever, are the experiences confined to isolated qualities. Rather they are complex, often highly so, but no more complex than the real experiences would be. A few words are gathered together here, in order to show clearly the presence of the several different qualities.

TABLE II.

Representative words yielding the various sense qualities.

Sweet: Dolly, Irving, joy, parlor.
Sour: Eunice, Inez, Italy, Josephine.
Salt: idle, Judith.
Bitter: amethyst, Browning, Harriet.
Cold: noise, Sarah, William.
Hot: cause, discrete.
Pressure: Ethel, Hall.
Pain: Eunice.
Tactual-motor: Ben, Bess, Clara, Kitty, John, Phœbe.

The color characterizations, as, for example, in the equivalents of Austrian, Benjamin, Dwight, Italy, and Ruth, are inserted solely for the purpose of more exactly describing the object referred to. They must not be taken to indicate the presence of subjective photisms accompanying the other experience.

These synæsthesias are often experienced in the ordinary course of listening to conversations, lectures, etc. Short sentences do not behave as units in producing 'tastes,' but sometimes the compound words will be effective, causing the appropriate 'tastes' to appear serially. Similarly, long words frequently behave in this fashion, parts of a word evoking quite distinct and separate 'tastes.'

The speed with which a word is pronounced is sometimes an important condition. Thus the word *light* pronounced quickly, with little accentuation on the vowel, equals "the soft, juicy lobe of a grape;" pronounced slowly, with vowel prolonged, it equals "chicken gravy." And *ease* gives "dry meat" when spoken quickly and sharply; while, if spoken more slowly, its equivalent is "something softer, pliable, creaky, like puffed rice." It follows from this that the same word has different 'tastes' when spoken by different individuals. There is no evidence, however, that in general vowels are the more influential parts of words.

The direction of the experience is always from sound to 'taste,' the reverse being possible only as the connections are remembered.

When requested to state the gustatory equivalent of a word, the subject will frequently pronounce the word to herself before making reply. Enquiry as to the purpose of this elicited the fact that such pronunciation serves often to reinforce the auditory excitation and thus make the gustatory qualities more vivid and more readily describable.

The best condition for the appearance of the synæsthesia seems to be a state of natural hunger. Thus a noon lecture arouses distracting and, since the 'tastes' are in no way satisfying, extremely tantalizing experiences, as does also table conversation before the subject herself is served with food. When faint or jaded, the synæsthesia is less pronounced. The presence of food in the mouth does not interfere with the vividness of the experience.

II.

With the exception of knowing that high piano notes gave a banana flavor, the subject was not aware that any sounds other than proper names and English words were involved in her synæsthesia until special tests were made. The following Tables present the results of these tests, in which nonsense syllables, foreign words, and non-vocal sounds were used. These Tables are self-explanatory and need no special comment.

TABLE III.

Further Gustatory Equivalents of Vocal Sounds.

Nonsense Syllables.[1]	
bik,	Something stiff and brittle.
bod,	Something sour with the texture of a pickle.
dep,	Meat, roast beef, well done.
dob,	Something soft, smooth like silk.
gur,	Sweet, like spice cake, crumby, dry.
hes,	Small particles, minced meat.
lor,	Liver.
niv,	Something with the texture of cooked pears.
sut,	Mutton fat.
tof,	Something sweet. (At back of mouth, on palate.)
vux,	Something stiff, about like Graham crackers.
yoz,	White part of egg beaten stiff. (Texture vivid.)
zaf,	Meat flavor, salty, hard, probably corned beef.
zod,	Something damp and solid.
Italian Words.	
canto,	Sweet, stiff, hard, splintery candy.
vita,	Something crunchy and sour.
nostra,	Something soft and agreeable.
cammin,	Something stiff and hard with caramel flavor.
del,	Something sweet, pleasant, cool, clear, delicate, like milk that has been poured over a baked apple.
mezzo,	Olive oil.
French Words.	
dont,	Something sweet, dry and rough.
exemple,	Something with the texture of felt.
une,	Something sour and juicy.

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[1] The following yielded nothing whatever: biv, dik, hed, lat, lim, mup, sax, tem, tez, vaz, vip, wal, wat, yez, yol, yuz.

mon*trer*,[1]	Custard-like, salty, an omelet.
*au*cune,	Dry fragments or crystals of something, like lumps of camphor.
German Words.	
{ Ge-	Gum.
{ stalten,	Something rich and sweet.
schwank*ende*,	The filling of squash pie, soft and sweet.
wieder,	Something stiff, with quality of pop-corn, mildly sour.
*ge*zeigt,	Something soft, not very pleasant.
Blick,	Something stiff, dry and tasteless, like raw macaroni.
{ trü-	Something sweet and delicate.
{ ben,	Something brittle between the teeth.
dem,	Cooked cocoanut.
einst,	Something a little salty.
sich,	Soft shreds of meat, moist and oily.
früh,	A clear sweet juice.
die,	Something stinging and fizzy.
zu,	Gravy.
fest,	Something sweet.
dies,	(Like English 'ease' and 'beast').
mal,	Melted butter.
wohl,	Something rich, overrich.
{ Ver-	Cooked apple, as in pie, drier than in baked apple.
{ such,	Gravy.

TABLE IV.

Gustatory Equivalents of Non-vocal Sounds.

Various Musical Instruments.	
A D tin whistle,	A clear sweet flavor, like Christmas candy or sugar and water. The higher the note, the less pronounced the sweet.
A C mouth organ,	Sweet and peppery at the lowest notes. The sweet increases for one-third of the upward range and then diminishes. The peppery flavor rapidly decreases and vanishes one-third of the way up.
Tuning forks,	
256 vibrations,	As if warm air were resting upon the tongue.
512-1024 "	Warm, and clear sweet; the former ceasing at the fourth note and the latter steadily diminishing.
Piano,	
A_2—E_1,	Like toast soaked in hot water.
E_1—F,	Sweet, rather strong (like licorice)—a troche.
F —g,	Mild, gravy-like.
g —c^4,	Banana (smooth, slippery).
c^4 — c^5,	Thin, insipid.
Violin,	
Lowest three notes,	Troche flavor.
From there up,	Grows sweeter, loses strength, becomes clear, delicate and sweet in flavor.

(Several other less important instruments gave results more or less similar.)

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[1] The italicized portions of words contributed nothing to the 'tastes' experienced.

Noises. Several dull, flat noises evoked nothing. The rubbing of a nail on a file evoked an experience of temperature in the mouth, this being hot or cold according to the kind or degree of scraping.

III.

But what evidence have we that an actual case of synæsthesia is here being reported, and not a case of artificial association due to a lively dramatic fancy? This is a point that must be raised, for it is very easy to entertain a suspicion that these phenomena are essentially ungenuine. Now, of course, in matters of this kind general impressions and personal knowledge of the subject count heavily. And on both these grounds I have no hesitation in asserting my conviction that the above-cited equivalents are the expression of a genuine synæsthesia. But since such personal impressions are sometimes unlikely to carry conviction to others, there are, fortunately, several considerations of a less personal nature which can be brought forward. The following facts are more compatible with the theory of synæsthesia than with that of mere suggested imagery. (1) The subject herself, an accurate, careful and discriminating person, testifies that the 'gustatory' part of the experience comes quite unsolicited. It is *found*, not manufactured. This, it is affirmed, is unquestionably true now and as far as can be recalled it has always been so. In support of this account of the matter the following instance is given as illustrative. The subject, while at work in the library, finds herself tasting roast beef. Casting about for the auditory cause, she hears the murmur of men's deep voices coming from an adjacent alcove. (2) The food-equivalent of the 'gustatory' qualities experienced must often be sought for with some diligence before an adequate description of the matter can be given to the questioner. That is, the experience and the naming of it in terms of a given food are quite distinct affairs. This search was repeatedly evident in the course of obtaining the several lists, and occasionally, in fact, only the qualities themselves were given. Added to this is the fact that, as the subject reports, "some words produce a 'taste' which I seem never to have experienced before either in the pronouncing of words or in eating. Thus the equivalent of *Buddha* (the only specific instance recalled by the subject) is said to be "vivid but indescribable," suggesting olives, but not to be designated accurately by any known food. These facts are to the subject herself most strongly confirmatory of the correctness of her introspective analysis. (3) Many of the experiences are given quite definite locations in the mouth. Thus the equivalents of *Ethel* and *Hall* (tactual) are felt at the tip of the tongue; of *tox* (irritation) at the back of the throat; of *Judith* (salt) at the sides of the tongue; of *Sarah* (cold) on the lips; of *amethyst* (bitter) "at the back of the mouth, on the roof, where the root of the tongue seems to hit it;" of *Hi* (cold) at the rear of the tongue; of *Miriam* (cool) on the anterior surface of the tongue; of *dice* (cool and salt) along the edges of the tongue; of *Florence* (sweet) over the tongue as a whole; of *Harriet* (bitter) across the rear of the tongue. As to the acid equivalent of a word, the subject says the effect is sometimes to "draw the jaws together." The sour is located along the edges and at the sides of the tongue. (4) When in doubt, the subject has a way of slightly pressing the cheeks inwards with the fingers until the satisfactory equivalent comes. This points to the sensory rather than to the imaginary character of the phenomenon. (5) The erratic nature of the matter, many words and sounds evoking no 'tastes' whatever, is precisely like that of the well known synæsthesias. (6) The subject testifies that the experience has a character intermediate between the reality of sensation and the unreality of fancy, imagined tastes seeming to be "in the head" rather than in the mouth. And (7) the equivalents possess a constancy which would hardly be possible apart from a true synæsthesia. After an interval of six months a number of words were given at random from the original list, with the result that the identical equivalents were described in almost precisely the same language.

IV.

In the hope of getting some basis for a theory of this case an examination of the data was next made with a view to ascertaining whether similarities of sound are connected with like 'gustatory' complexes. I may say at once that no great success attended this examination. (a) As a preliminary experiment I chose certain monosyllables, as given in the list below, representative of labial, dental, and other word elements, and pronounced them as distinctly as possible to the subject. The variety of 'gustatory' qualities given under each class shows that *articulatory similarity is not paralleled by 'gustatory' similarity*. This negative result is not, however, without value, since it may be taken to be confirmatory of the fact that the true excitement of the 'gustatory' qualities is auditory and not articulatory in character.

TABLE V.

Equivalents of Words Similar in Articulatory Character.

	Babe,	Elusive, cool.
	Pipe,	Cool.
(Labial)	Maim,	Quite salty. Fumes pass through the nose.

(Labial)	Valve,	Soft. Melted molasses candy.
	Fife,	Cool, slightly sour.
	Did,	Stewed tomato.
	Cease=ees=ease,	(See Table I).
(Dental)	Thin,	Not definite.
	Tent=content,	Stiff, sharp, candy.
	Church,	Meat, soft, lamb.
(Palatal)	Cake,	Like itself, but stiff cake. Cool.
	Gag,	Nothing.
(Nasal)	Sing,	Gingerbread, spicy, warm.
	Nun,	Strong. Dry baked beans, à la New York.
(Lingual)	Lul,	Trifle sour.
	Far,	Meat, beefsteak, not very juicy.

(b) The auditory similarities yielded, upon examination, a few fairly well marked correlations, but not a sufficient number to be at all satisfactory. Occasionally a prominent vowel sound seems to be effective in various settings. Thus *Oo!*, *who*, and *Susan* all suggest the eating of milk-toast; *he*, *ease*, and *Cæsar*, dry meat; *William*, and the first two syllables of *Fillebrown*, water; *Edna*, *Edgar*, and *Edward*, boiled eggs; *Bess*, *Tress*, *Jessie*, small pieces of something (*Phœbe*, however, does the same); *joy*, *Joyce*, *rejoice*, *Royce*, and *Marjorie*, rich cream candy (though *boy* 'tastes' like gum-drops). On the other hand *Dolly* does not wholly agree with *Polly* and *Molly;* and *Hi!*, *dice*, *fife*, *idle*, *Inez*, and *pipe*, though suggesting cold more than any other quality, are connected irregularly with both salt and sour. Still more distracting is it to find that identical 'tastes' are joined to very diverse sounds. Thus one 'gustatory' equivalent does duty for all members of each of the following groups: *Henry*, *Hope*, *Stella; Dora*, *men*, *women; Alfred*, *Elsie* (though *Albert* is indefinite); *Newcomb*, *Rachel; Florence*, *Mabelle; Dorothy*, *Gladys; Louise*, *Tabby*. From a careful examination of the entire list from which the above examples are taken I am convinced that it is idle to seek any rigid uniformity of connection between given auditory qualities and their 'gustatory' equivalents. And, indeed, perhaps the very complexity of both items of the experience would render this search fruitless, even if definite correlations existed.

V.

In attempting to decide whether the above-cited experiences are to be explained by the physiological or by the psychological theory, we are, apparently, in no better and in no worse case than in respect to all varieties of synæsthesia. No decisive facts are at hand. In this regard and in all essential respects, the phenomena here are typical, as any one acquainted with the literature of synæsthesia must have recognized. To conjecture that the subject's sensory defects—deafness and anosmia—are directly or indirectly responsible for this special case, these being perhaps indications of a more or less extensive cross-circuiting of cortical currents in the regions concerned, is, after all, to indulge in mere speculation. It is, however, alluring to think that the slowness of hearing is due to the deflection of neural tensions from the appropriate auditory centres of the cortex into those regions that subserve the sense impressions of the mouth. This conjecture would be substantiated in a measure if it could be found that a decrease of auditory acuteness is accompanied by an increase in the number and vividness of the synæsthetic experiences. For upwards of a year the subject has had this particular problem before her, and although an increased vividness of the 'gustatory' phenomena does sometimes accompany the diminished acuteness of hearing, she is not willing to assert that this is not due to a more careful directing of the attention upon these phenomena. As the matter stands, therefore, this particular conjecture must be looked upon at present as lacking the desired support. Nevertheless, one who inclines, as does the present writer, to the physiological as contrasted with the psychological explanation of synæsthesias in general may find good evidence for the former, it seems to me, in the facts above cited concerning the constancy of the gustatory equivalents; the dependency of the experience upon individual fashions of pronunciation; the repeated difficulties in putting the experience, itself perfectly definite and vivid, into words which adequately describe it; and, lastly, the frequency with which the 'tastes' can be given precise localizations within the mouth. Still, here no less than in all known cases of synæsthesis, we can only regret that our theory is so lame while our facts are so secure.

THE PERIOD OF MENTAL RECONSTRUCTION.

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The following account is a statistical study of some of the experiences that are met along the highway toward intellectual maturity. The studies of Leuba[1], Starbuck[2], and Coe[3], have thrown much light on the various ways in which the religious life develops, but less has been done for the corresponding aspects of the intellectual life. Burnham,[4] has published some data on the place of doubt in intellectual and religious development, and Starbuck more or less incidentally gathered some data on what he called the period of reconstruction.[5] His returns showed that some people experience subjective transformations of varying degrees of intensity quite apart from specific religious development. Such people are at times keenly conscious of experiencing far-reaching changes or enlargements in their intellectual points of view. It is with these transformations, their causes, their presence or absence, and the comparative frequency of various types that this study is concerned.

The study is based on returns obtained by means of the following questionnaire, supplemented by correspondence and personal interviews. Three hundred twenty-seven answers to the questionnaire were received, but of these only 282 were used in preparing this paper. The purpose in rejecting the others was to avoid the fallacy of selection. The 282 came from groups in which very nearly all answered.

QUESTIONNAIRE.

Read All the Questions Before Answering Any.

Please answer by placing "yes" or "no" before the question or statement.

Mark the degree of certitude of all your answers. If you are sure of your answer, mark it A. If fairly sure, mark it B. If in much doubt, mark it C. If it is only a random guess, mark it D.

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[1] A Study in the Psychology of Religious Phenomena, *American Journal of Psychology*, Vol. VII, p. 309 f.

[2] *The Psychology of Religion*, London and New York, 1899. Also *American Journal of Psychology*, Vols. VIII and IX.

[3] *The Spiritual Life*, New York, 1900. Also *Psychological Review*, Vol. VI, p. 484 f.

[4] The Study of Adolescence, *Pedagogical Seminary*, Vol. I, p. 175 f.

[5] *Op. cit.*, chapter XXII.

The data obtained by means of this questionnaire are to be used for scientific purposes and are strictly confidential.

I. Consider the history of your mental (intellectual and moral) development. Which of the following has it been most like?

(II) 1. My mental development has been on the whole gradual, but several incidents in my life have given it noticeable impetus, without, however, introducing any great changes.

(V) 2. In my mental development I experienced one pronounced awakening or transition that gave me a point of view and unified my conception of things, but otherwise my mental development has been gradual.

(I) 3. My mental development has been gradual, so that my attention has never been attracted by subjective changes or transitions.

(IV) 4. In my mental development I have experienced one pronounced awakening or transformation that gave me a point of view and unified my conception of things. Since then, and to some extent before, I have been conscious of a number of smaller transitions which gave me new points of view and deeper insight.

(III) 5. In my mental development I have experienced a small number (say 2-10), well marked transitions. I have felt at irregular intervals that I had gained new and important points of view.

II. If, in your mental development, you have experienced a noticeable or pronounced transition:

1. What was your age when it occurred?
2. How long did it last?
3. Was it preceded or accompanied by doubt? By depression?
4. Was it preceded or accompanied by a calm state of mind? By elation?
5. In the transition, did you feel secure and that your mental foundations were becoming firmer?
6. In the transition, did you feel that old foundations were crumbling and that you were 'at sea'?
7. Was the transition followed by doubt? By depression?
8. Was the transition followed by a calm state of mind? By elation?
9. Was the transition followed by a rejection of former beliefs?
10. Was the transition followed by a readjustment of former beliefs?
11. Was the transition followed by consciously holding contradictory positions in science, religion, etc.?

III. By which of the following was this transition in your mental development most influenced? Mark with an X the one of the following that influenced you most; with a Y the next; with a Z the next:

1. The reading of any particular book or books. (If so, give name.)
2. The study of any particular subject. (If so, name it.)
3. The influence of a person. (If so, a teacher, pastor or who?)
4. Entering college.
5. Joining the church.
6. The death of a relative.
7.

IV.

1. Were you in childhood, before the age of twelve, taught religious beliefs or dogmas? If so, name church.
2. Did you receive little or no dogmatic religious instruction in childhood?
3. Have you experienced religious conversion? At what age

V. What was your occupation or schooling:

1. Between 10 and 13?
2. " 13 " ?
3. " " ?
4. " " ?
5. Between and ?
6. " " ?
7. " " ?
8. " " ?

(Fill out the blanks to suit your case, bringing it down to the present.)

VI.

1. What is your sex?
2. What is your present occupation?

Date..Name..

(If signing would keep you from answering frankly, do not sign.)

The returns that were used fall into two main groups, based on the immediate occupation in which those answering were engaged, and on the manner in which the returns were obtained. Those in one group were graduate students when they answered, and those in the other were teachers. I shall designate these groups respectively 'Graduate Students' (G. S.) and 'Teachers' (T).

The G. S. group comprises 170 returns in all. Eight of these were obtained through the kind co-operation of Professor W. C. Gore of the University of Chicago, 17 came from a section of the senior class in Teachers College, and the remaining 145 came from graduate students in Columbia University, mostly students in Teachers College. These were nearly all obtained during class time. With but one exception, returns were thus received from all to whom the questionnaire was submitted.

The returns from the group designated 'Teachers' came in the main from high school, normal school, and college teachers, and number 112 returns. Fourteen of these came from the Winona, Minn., High School, having been kindly obtained for me by Prin. W. A. Bartlett and Supt. C. R. Frazier. The remaining 98 I obtained from friends and acquaintances by mail. I first made a list of 50,—25 men and 25 women,—to each of whom I wrote a personal letter at the time the questionnaire was submitted. Forty-eight, or 96 per cent., sent returns. Having been so successful with this group, I repeated the effort with an additional group of 58. Of these 50, or 86 per cent., sent returns. The percentages of the different types of experiences in this group correspond so closely to those of the group of fifty, and to those of the G. S. that I feel safe in using them. Without a criterion with which to test them I should not have used them in this part of the study. Whatever the cause of the selection may have been, it evidently was not a certain type of experience called for by the questionnaire.

Although the 282 returns used in this study are practically unselected, in the technical sense of that term, the group as a whole is highly selected. It is restricted to what may be called the upper stratum of the teaching profession. Nearly half of those answering were actually engaged in teaching, and the others had been in that work and were preparing to go back into it.

This restriction was intentional. It gives us a group that is homogeneous and so may be compared with other homogeneous groups. It would be interesting to know, for example, how the clerical, the medical, and the legal professions compare with the teaching profession in the experiences here represented.

In presenting the data I shall follow the order in which they were called for by the questionnaire. The fallacy of selection having been avoided and the numbers being fairly large, quantitive statements may be given.

The data most fundamental in the study were elicited by the first topic of the questionnaire. It called for the experience of reconstruction, with which the study is primarily concerned. The five sub-heads, or classes, under this topic form a graduated series, the order of which is indicated by the Roman numerals in parentheses. These numerals were not printed on the questionnaire as used. The classes were intentionally mixed up so as to insure a more unbiased selection. It was thought that the person answering would thus be less likely to place himself at the median and be more likely to select the type of experience that most nearly fitted his case.

The experiences coming under this head were divided into five classes, not because it was thought that this exhausted the possibilities, but because that number of differentiations could easily be made and because it seemed to cover the field with sufficient minuteness. Actually, there are probably no discrete classes at all. It is more likely that the different degrees of intensity of the experience form a continuous series, approximating the normal probability curve in its form of distribution. The data present considerable internal evidence that this is the case, the mode falling about midway between Classes II and III.

The data obtained under this topic are given in Table I. They are given separately for the sexes and for the two main groups above indicated. As only six, three men and three women, placed themselves in Class V, these are here, as throughout the study, included in Class IV, from which they do not materially differ.

The different sections of the table agree tolerably well among themselves. The variations are not greater than might be expected from the number of returns used. The four classes are all nearly the same size throughout, Class II alone showing a tendency to be larger than the others. This tendency, how-

TABLE I.

Class.	Men		Women.		Total	
	Number	Percent	Number	Percent	Number	Percent
			Graduate Students			
I	25	26	12	16	37	*22*
II	33	35	31	42	64	*37*
III	18	19	16	21	34	*20*
IV	19	20	16	21	35	*21*
			Teachers.			
I	15	26	13	24	28	*25*
II	15	26	15	28	30	*27*
III	17	29	12	22	29	*26*
IV	11	19	14	26	25	*22*
			G. S. and T.			
I	40	26	25	20	65	*23*
II	48	31	46	35	94	*34*
III	35	23	28	22	63	*22*
IV	30	20	30	23	60	*21*

ever, is manifest chiefly in the G. S. group, which suggests that it may be explained in part on the basis of maturity.

Age was not directly called for, but it was usually given under Topic V. When it was not so given I succeeded nearly always in ascertaining it from other sources. The average age in the G. S. group was approximately twenty-nine years, and that of the T. group was thirty-three years. This difference of four years would enable a relatively larger number from Class II of the former to pass over into Classes III and IV than would be likely to be the case in the latter. The fact that the returns from the G. S. group were marked more hastily than the others, owing to a limitation of the time to twenty-five minutes, may also have tended to swell Class II. This class appeared first on the list, which exposed it to the factor of primacy.

No marked sex difference is manifest from the table. If any exists, it again occurs in Class II which is quite uniformly larger for the women than for the men. If an explanation for this is necessary, it cannot be sought in maturity, for the women average about the same age as the men. It would be explained on the assumption that women are more conservative than men, so that they do not give themselves up so readily to rapid and far-reaching mental transformations. As some of the data to be taken up later indicate such a difference, this point will be referred to again.

Psychologically, of course, these four types of mental development are equally significant. They are, perhaps, all equally normal and one leads to culture and insight as well as another, a statement well borne out by the data. There are returns from people of distinction in science and philosophy in all four classes.

Although these four classes are all equally significant as types of mental development, they are not equally spectacular. Classes III and IV are more picturesque than I and II. Descriptions of subjective experiences were not called for, aside from those briefly given in the questionnaire, nevertheless, a number of those who sent returns by mail gave such descriptions, and a few others I obtained from members of Class IV on request. It would consume too much space to publish them all, or even many of them, but two of Class IV are inserted. It must not be inferred, however, that all the experiences even in this class are as definite and clear-cut as those cited.

> Up to the age of nineteen or twenty I had retained a certain orthodox view of the world, though never a church member. But my interest in scientific reading upset me. I went through somewhat the disintegrating crisis described in *Robert Elsmere*, and the reading of the book brought me to the constructive outcome where the higher relations of religion and science seemed clear.

> Although I had been given little religious instruction in youth, I absorbed an 'orthodox' view of the world which I had no occasion to question. At the age of twenty-two the doctrine of evolution, the writings of Horatio W. Dresser, and the stimulating influence of a teacher gave me new insight. I obtained a conception of the world as an evolving, progressing unity, and I felt that I now understood. The 'orthodox' views caused some friction, but on the whole the accompanying emotional tone was one of elation. To make the major readjustments took me about one year.

The following citation from John Stuart Mill's *Autobiography*, page 66, is an excellent illustration of the extreme type of the reconstructive experience.

> When I had laid down the last volume of the traité[1] I had become a different being. The 'principle of utility' understood as Bentham understood it, and applied in the manner in which he applied it through these three volumes, fell exactly into its place as the keystone which held together the detached and fragmentary component parts of my knowledge and beliefs. It gave unity to my conception of things. I now had opinions; a creed, a doctrine, a philosophy; in one among the best senses of the word, a religion.

The experiences in Class III resemble those in Class IV. They are usually less acute, although not always, and include a smaller section of the mental life, thus giving opportunity for several to occur. One person distinctly described seven and another five, but generally the number is smaller. To make this phase of the matter concrete, a number of first-hand descriptions should be cited from each class, but space does not permit this.

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[1] Dumont's *Traité de Legislation*, three volumes.

The experiences of Classes II and I are progressively less pronounced than those of the other two classes. People who placed themselves in Class II were conscious of times when their mental growth was accelerated, when they came upon new unifying principles, but the matter made little impression on them. The people in Class I realize that their points of view have changed in the course of time, but they can point to no specific influences or occasions that have brought about the change. For example, one person responding by mail said:

> "My growth has been gradual,—no sudden transitions or changes. . . . . I need to think a matter over long before I adopt a new notion. By that time I often forget when I got it."

Topics II and III of the question list were meant to apply mainly to Class IV. Class I was by the nature of the case excluded, but nearly all the members of Class III supplied data, and many of Class II did.

The data elicited by the first two questions in the second topic can be satisfactorily considered only in relation to Class IV, although members of Classes II and III also attempted to answer them. These questions request (1) the age at which the transition occurred and (2) the length of time it lasted. All the members of Class IV, thirty men and thirty women, answered the first question, and twenty-one men and twenty-four women gave specific times under the second.

The ages at which the transition occurred vary for the men from 14-42 years, with an average of 22.2 years and a median of 21 years. Forty-seven per cent. of the ages fall between 18 and 22 inclusive, 63 per cent. between 18 and 25 inclusive, and 16⅔ per cent. fall below 18 years. For the women the ages vary from 13 to 32 years, with an average of 20.25 years and a median of 19.5 years. Fifty-three per cent. of the ages lie between 18 and 20 inclusive, 70 per cent. between 18 and 22, and only 10 per cent. fall below 18 years. The men show a greater range of variation than the women and average nearly two years older. This difference agrees well with the difference in the time of the advent of puberty in the two sexes, which indicates that the period of reconstruction is likewise a function of maturity, in part at least.

The average ages found by Starbuck are higher than those given here.[1] He found them to be 24 years for the women and

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[1] *Op cit.*, p. 279.

24.5 years for the men. How to account for this difference is not clear. The fact that the ages in Starbuck's returns were given incidentally may account for it in part, a person having met the experience late being more likely to mention it; and it may in part also be owing to the fact that he studied miscellaneous groups. A person taking a college course and following an intellectual calling may meet this period earlier than one who does not.

The length of time occupied by the transition is not so easily given as the age at which it occurred. Few, if any, had any hesitation in saying when reconstruction began, but many found it difficult to say when it ended. The times given by the men vary from a month, or less, to three years, and those given by the women vary from three months to four years. Seventeen per cent. of the men and ten per cent. of the women indicated that the experience is continuous. One-third of these admitted that they were in the midst of the process. The remaining two-thirds may either be similarly explained, or they merely continue to feel the elation coming from a unified mental life.

The average time spent in the transition by the men was 1.6 years and by the women 2.1. The women evidently take a longer time to adjust themselves to the new point of view than the men. This is in harmony with the common observation that women are, as a rule, more conservative than men. In the reconstruction, usually long cherished, but now inconsistent views, must be modified or discarded, and although the women are intellectually convinced of the new truths, their feelings linger.

The ages at which the transition occurred as given by Class III are not so reliable as those given by Class IV, where all answered the question, but they may be mentioned in passing. Fifty-one ages were given by the men and 25 by the women, coming in each case from about half that number of persons. The ages vary from 8-40 for the men, with an average of 20, and from 10-32 for the women, with an average of 20.1.

The data gathered by questions 3-11 under Topic II are condensed in Table II. The percentages of the positive answers only are given. Many negative answers were also recorded, but the tendency is for people to leave a blank where a positive answer cannot be given.

It is clear that all the features having to do with 'storm and stress' increase as we pass from Class I to Class IV. In fact, the column for Class I is practically blank.

A marked falling off in doubt and depression is noticed after the crisis has been passed, and there is a corresponding increase

TABLE II.

	Men					Women					
Class	I	II	III	IV	Total	I	II	III	IV	Total	M. & W.
During transition.											
Doubt		15	63	77	34		13	36	70	29	32
Depression		6	43	53	22		6	29	60	22	22
Calm		21	34	47	23		8	29	33	17	20
Elation		10	23	23	13		2	11	33	11	12
Secure		35	63	70	38		17	53	50	30	35
'At sea'		10	51	60	27		6	32	63	24	26
After transition.											
Doubt		8	37	20	15		6	18	37	15	15
Depression			14	17	7			14	27	9	8
Calm		29	54	63	34		13	45	47	26	30
Elation		13	37	43	21		4	29	43	18	20
During and after transition.											
Rejection		21	43	50	26		6	21	43	17	22
Readjustment		35	78	80	44	4	19	53	70	36	40
Contradictory		8	20	23	12		2	11	47	14	13

in calm and elation.[1] Those who were afflicted with doubt and depression during the transition frequently gave a positive answer also under 'calm,' and sometimes even under 'elation,' but they usually indicated that there was an alternation of moods. A similar alternation was also frequently indicated between feeling secure and feeling 'at sea.'

'Storm and stress' is not a necessary accompaniment even in Class IV. Eighteen per cent. of this class report that they experienced no doubt, depression, or feeling of 'at sea'; this in spite of the fact that 64 per cent. of this 18 per cent. had received religious instruction and that 73 per cent. had readjusted their religious beliefs. These percentages are almost identical with those of Class IV as a whole, which are 65 per cent. for religious instruction and 75 per cent. for readjustment of beliefs. This is strange when it is noted that it is usually the incompatibility between early religious teachings and later scientific and philosophic instruction that causes the friction. What it seems to indicate is that storm and stress is, in the main, a function of temperament. No matter what mental readjustments have to be made, some people do this with little or no emotional disturbance.

The intensity of the storm and stress period in those that do experience it varies greatly. Although not called for, three mentioned that it was very painful, and two of these had contemplated suicide. Taking the rubric 'doubt' as a criterion of storm and stress, 32 per cent. of the people studied passed through such a period. This figure may be slightly too low as 13 in Classes III and IV, making 5 per cent. of the total number, left Topic II unanswered, but the percentage would certainly not rise above 38 or 40. This is not necessarily at variance with the assertion of Starbuck (*o. c.* p. 214.) that over 60 per cent. of average American young people pass through such a period. His figures include the storm and stress frequently experienced at the onset of adolescence, while ours include only the period of reconstruction.

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[1] A number of those that answered the questionnaire suggested that the word 'satisfaction' be substituted for 'elation.' Had this been done, the caption would no doubt have drawn a still larger percentage of positive answers.

Questions 9, 10, and 11 are not specifically 'storm and stress' questions and so require separate mention. Seventeen per cent. in Class IV indicated that they rejected former beliefs entirely. Seven per cent. did so in Class III, and 2 per cent. in Class II. The others who answered that question in the affirmative also answered the next question, usually indicating that there was partial rejection and partial readjustment. The large number indicating a readjustment of former beliefs shows this to be a constant accompaniment of mental reconstruction. The fact that it is so would lead one to think that certain types of religious and other dogmatic instruction are responsible for at least some of the storm and stress that exists.

It is not explicitly revealed by this study to what extent storm and stress is a phenomenon of individual, and to what extent of social psychology; to what extent it is a result of the process of growth and development, and to what extent it is a result of instruction and environment during childhood.

No matter how narrow and dogmatic the instruction has been, a certain type of temperament seems to be necessary to bring about emotional disturbances later. This is evidenced by the fact mentioned above, that some people who have had much dogmatic instruction readjust their beliefs without storm and stress, and by the figures of Table IV below. The percentages of those having received much religious instruction are nearly the same in all four classes.

No striking sex differences are revealed in Table II. In the last three items, however, the figures are quite uniformly divergent for the sexes, which would indicate the existence of such differences. Twenty-six per cent. of the men rejected in whole or in part their former beliefs, as against 17 per cent. of the women; and 44 per cent. of the men readjusted their beliefs, as against 36 per cent. of the women. This rejection and readjustment of long cherished beliefs are radical acts, and the fact that the women do this less than the men is again in harmony with the assumption that they are more conservative. Still, this difference in the figures might be explained otherwise, an explanation, however, that does not seem to me to have much weight. It is indicated in Table IV that the women of this group have received slightly less religious instruction than the men, and in Table III that they are less attracted by science and philosophy and therefore study them less. But it is these subjects that are most likely to unsettle certain forms of beliefs.

The average percentages for holding contradictory beliefs are 12 for the men and 13 for the women. But in Class IV, where the issue is clearest, the percentages are respectively 23 and 47, indicating that in this class the practice is fully twice as frequent among the women as among the men. This would indicate again the more conservative nature of women. They find it hard to discard former beliefs of whose inconsistency they have nevertheless become intellectually convinced.

In drawing this inference, I have not forgotten that just the opposite conclusion could be drawn from the figures under Classes II and III. I noticed frequently in gathering the data that the women were much more cautious, and apparently secretive, in marking this item than the men. They appeared unwilling to confess that they had held contradictory beliefs. Some answered the question in the affirmative and then erased the answer. This indicates that all the figures from the women should be higher in order to make them comparable with those from the men.

The causes influential in bringing about the transition were elicited by the rubrics under Topic III, and are condensed in Table III. The percentages are based upon the entire number of entries that were made. In all there were 490 of these, 253 by the men and 237 by the women. Counting only those who made entries, this gives an average of 2.4 per man and 2.5 per woman.

TABLE III.

	Men	Women
Personalities..................	35 per cent.	45.4 per cent.
Science and Philosophy......	33 "	18 "
Psychology...................	4 "	3.4 "
Literature....................	5.6 "	8.3 "
Entering College.............	8.6 "	16.3 "
Joining Church...............	6 "	2.6 "
Miscellaneous................	7.8 "	6 "

In classifying these entries I made originally ten classes, respectively for (1) Personalities, (2) Philosophy, (3) Science, (4) Social Science, (5) Bible and Science of Religion, (6) Psychology, (7) Literature, (8) Entering College, (9) Joining Church, and (10) Miscellaneous. Under the last head I placed unnamed items as "a book" or "a subject of study," and such subjects as "travel" that were mentioned but seldom and would not logically fall in the classes made. Deaths, which composed 3 per cent. of the influences for the men and 6 per cent. for the women, were classified under personalities, regarding them as the subtraction of personalities from the environment. The figures for philosophy and the different sciences, with the possible exception of psychology, all showed the same trend and so they were combined.

The sense in which these things may be regarded as causes of the transition cannot be expressed in a sentence. They differ in this respect both for the items themselves and for the people mentioning them. A personality, as a teacher, may do one thing for one person and quite a different thing for another person, and its influence may never be like that of metaphysics, for example. A teacher in the classroom might supply the principle for reconstruction, or he might merely suggest a fruitful line of reading. In conversation, in letters, and on the blanks, the statement was frequently made that the items mentioned should not be regarded as sufficient causes, but rather as inciting causes. They precipitated a reconstruction for which the mind had long been getting ready through study and experience. The particular influence serves usually to supply, or lead one to find, a unifying principle for things that are already in the mind, but which have heretofore been more or less unrelated. The development is at bottom an educational one that has become ripe for integration. In itself probably no influence has power to reconstruct. To be efficacious, the mind must be ready for it, and what would influence one might not influence another.

The difference between the people that are conscious of transitions and those that are not is probably in the main temperamental. This means that they instinctively react differently to similar situations. Some of the persons in Classes I and II are so conservative, or cautious, or reactionary regarding new ideas that they do not adopt them outright, but 'get used' to them so gradually that they are never really conscious of how they got them. Others again may be quite the opposite in attitude and continually reconstruct their mental content as they pass along the highway of learning and experience, so that large transitions are precluded. Other attitudes, or native and acquired mental equipments, might be posited to explain all the various types of experience in this respect.

A perusal of Table III reveals a number of sex differences that are worth noting. As would be expected, personalities count more with women than with men. They form nearly one-half the influences mentioned by the women and only about one-third of those mentioned by the men. Teachers appear to exert about equal influence over the sexes. They comprised 18 per cent. of the items for the men and 21 per cent. for the women. This leaves a balance of 17 per cent. and 24 per cent. for personalities closer than teachers, such as parents, brothers, sisters and friends, or a ratio of 3-2 in favor of the women. Science and philosophy, on the other hand, count more with men than with women, in the ratio of nearly 2-1. The fact that men study these subjects more than women, and therefore are influenced more by them does not explain away this difference in the figures, but points in the same direction. The reason why men study them more is not so much conventional as it is because they are more attracted to them. Literature is not given as a cause as much more frequently by the women than by the men as might have been expected. But when it is known that Emerson and Browning are the authors most frequently mentioned, this may be accounted for. The writings of these authors might almost as well have been classed with philosophy, for they supply the same kind of unifying principles. Entering college, by which those answering meant either the event of going off to college or the influence of the college course, appears decidedly more momentous for women than for men. This is very likely owing to the fact that it forms more of a contrast in their lives than it does in those of the men. Joining church, on the other hand, impresses the men more than the women. The explanation is probably similar to that of the preceding, but is reversed for the sexes. Boys are more likely than girls to get away from the influence of the church, and so coming back and joining it forms more of a step for them.

The fact that the relative amount of influence exerted by the various items was called for has not been taken into consideration in the preceding discussion. It was faithfully given but each item was assigned with about equal frequency to first, second and third places, so that weighting seemed unnecessary.

Topic IV, pertaining to religious instruction and to conversion, was included especially for the purpose of separating the religious experiences from those that are more purely intellectual. That the period of mental reconstruction is a phenomenon independent of religious conversion is indicated by the data gathered by question 3 under this topic. Unfortunately the second part of this question, calling for the age of conversion, was not on the paper at first. I added it after more than half the data had been collected. Nevertheless I got the ages o conversion from 32 men and from 20 women. These range fr the men from 11 to 24 years with an average of 16 and a media of 15, and for the women from 10 to 25, with an average of 14 and a median of 14. For the men there seem to be two modes one at 13-14 and one at 18, and for the women there is one a 14. All these figures show that conversion is an earlier phe nomenon than mental integration, on the average nearly si years earlier. The same fact also came to light in Starbuck' returns. In Class IV, I obtained the age of conversion from 1 or from 20 per cent. In only two of these instances did th ages for the two experiences coincide.

TABLE IV.

	Religious Instruction			Conversion		
Class	Men	Women	Total	Men	Women	Total
I	60	48	55	35	16	28
II	69	59	64	50	22	36
III	71	75	73	46	54	51
IV	70	60	65	50	37	43
Aver.	69	61	64	45	31	39

The rest of the data gathered by this topic are summariz in Table IV. It has already been pointed out that the amou of religious instruction received in childhood does not correla in any significant way with the experiences of mental reco struction. Some correlation exists, however, between ment reconstruction and the experience of conversion. This is i harmony with the assumption that the persons falling towar Class I in the curve of distribution adjust themselves mor gradually and with less subjective upheaval in all things They are more calm and deliberate in temperament than thos at the other end of the curve, who are more emotional.

In all, 69 per cent. of the men and 61 per cent. of the wome said that they had received much religious instruction in child hood, and 45 per cent. of the men and 31 per cent. of the wome had experienced religious conversion. Why the men shoul experience conversion more frequently than the women seem to require explanation, for women being more emotional, an presumably more religious than men, the opposite would natur ally be expected. In part, the explanation very probably agai strikes back to the fact that boys get away from church an home influence more than girls. After having sowed som 'wild oats' they come back to their former sphere of life, pass ing through the process of conversion in doing so. Girls pass out of the influence of the home and the church less frequently than boys and so have less occasion to become converted. The greater conservatism of women may also enter into the explanation. They are less given to abrupt changes than men.[1]

In all 110 answered as having experienced conversion. Seventy-eight per cent. of these had received much religious instruction and 22 per cent. little or none. Although the opposite might again be expected, the explanation is probably not far to seek. To have received religious instruction means to have been brought up in a religious home. The members of such a home are most likely to come under the influence of church services, revival meetings, and other factors that stimulate conversion. No doubt heredity is also a factor. The predisposition in church-going families for deep religious experiences may be inherited like other traits. Furthermore, religious instruction in childhood is taken much as a matter of course, having little meaning attached to it, hence when conditions become ripe to give it content, the experience is likely to overwhelm the mind. All former instruction appears in a new light and gets a significance essentially new. But this cannot well be the case with those that have received little or no such instruction. They have nothing at hand to be made meaningful and so are more free to go about the matter intellectually and become adjusted gradually to the deeper meanings that the experiences of life unfold.

Topic V calls for biographical items pertaining to occupation and schooling. It was thought that irregular schooling, or getting one's education late in life, might be a cause contributing to transitions consciously experienced. A few instances point to the conclusion that this may be a subsidiary factor, but it certainly does not enter into many cases.

It will be of interest to give a brief account of the returns that were not included in the foregoing data. These are condensed in Table V.

Fifty blanks, together with a return stamped envelope and a request for a reply, were sent to Methodist ministers, and the same number to members of the Chicago Board of Trade. The names in each case were taken in alphabetical order from published lists. Eight in each group returned the blanks unanswered, while twelve clergymen and three business men kindly supplied data. The remaining thirty clergymen and thirty-nine business men were not heard from.

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[1] It is not meant, of course, that all women are more conservative than men, but that they are more so on the average. It might well be that the most conservative person in the world is a man, and the most radical person a woman.

TABLE V.

Class	I	II	III	IV	Blank
Clergymen	1		3	8	8
Business Men	1	2			8
Teachers	4	2	1	1	1
Miscellaneous	4	9	1	8	
Total	10	13	15	17	17

The fact that all but one of the clergymen that answered had passed through periods of reconstruction is no doubt significant, and it is to be regretted that all did not supply data. It may be equally significant that none of the business men answering had experienced reconstruction, but three replies is manifestly too small a number to bear much weight.

Fifty blanks were also sent to Wisconsin high school teachers, but with an unstamped return envelope. Nine replies were received, as given in the table. I wish now that I had stamped the envelopes so as to make the data more nearly comparable with those from the clergymen and from the business men. The fact that my supply of blanks gave out (600) made it impracticable to extend the distribution.

The miscellaneous group comes from two city superintendents and from a friend at Columbia University. To the superintendents, who had expressed a willingness to assist me by submitting the blanks to their high school teachers and their grade principles, I sent 25 to one and 75 to the other, and received in return 5 and 12 respectively. The remaining five returns, collected by the friend above mentioned, came from people that were neither teachers nor graduate students. Of five requests for assistance sent to university people, only one brought a favorable response.

The data of Table V were obtained under such a variety of conditions and the numbers in each group are so small that little can be inferred regarding selective influences. They do indicate, however, that reliable questionnaire data can be obtained only under exceptional conditions, such as were mine at Columbia University. In addition to returns from friends and acquaintances, one must be able to get people in groups, such as are afforded by school or college classes. But even then it takes no less pains to gather reliable data by means of the questionnaire method than by any other legitimate method. That reliable data may be gathered in certain departments of psychology by means of the questionnaire method, supplemented by correspondence and personal interviews, is borne out by the results obtained by Galton and others. The method does not apply everywhere in psychology, but neither does any other method apply everywhere. Taking it for granted that the subject of this study was worth investigating, by what other method could this have been done?

The reliability of measurements based on such methods as 'right and wrong cases' and 'average error' may be calculated mathematically, but this is not so readily done with questionnaire data. One may obtain the respondent's own degree of confidence in his answers, but just what this is worth is not known; and one may get him to repeat his answers after he has forgotten how he answered on the former occasion, and then compare the consistency of the results. In fact, one aspect of the reliability of a person's answer might, and should, be extensively investigated by the second method suggested.

TABLE VI.

Certitude Blank			A		B		C		D	
Topic	No.	%	No.	%	No.	%	No.	%	No.	%
I	61	21·5	158	56	59	21	4	1·5		
II	619	45	551	40	175	12·5	29	2	7	·5
III	302	68	94	21	36	8	14	3	1	
IV	435	69	177	28	14	3	1		1	

On the accompanying questionnaire the request was made of the person answering to indicate his degree of certitude. He was requested to mark his answers A, B, C, or D, according as he was sure, fairly sure, in much doubt, or making a random guess. The data, in probably a too much condensed form, are given in Table VI. As far as certitude was indicated, it is of an unexpectedly high degree. Comparatively few answers were marked C or D. Under Topic I all the C's and 63% of the B's were given by members of Classes I and II, those that had experienced no pronounced transitions. The degrees of certitude of the separate questions under the other topics were also not distributed in equal proportions.

On the average approximately half the answers were left blank as to certitude. About ten per cent. of these I investigated by means of personal interviews and found that nearly all felt certain and wished their answers to be marked A. Only two asked to have theirs marked B. I received the impression that when people are certain they feel that they need not indicate that fact.

Before the questionnaire was printed in the form in which it was used, a trial copy was prepared and mimeographed. This varied but little from the form that was used, and was answered by twenty-seven persons that later also answered the printed copy. The influence of memory from the first to the second report was slight because an interval of two months elapsed between the two, and none of the people expected to have the blank submitted to them again.

Comparing the answers in Topic I, 16 of the 27 were alike in the two returns, 6 had checked adjacent classes, *i. e.*, they had, for example, moved from Class I to Class II, or *vice versa*, and 5 had checked in such a way that the two returns seemed clearly discrepant. But when the answers to Topics II, III and IV were compared the discrepancies between the two returns were quite inconsiderable. Practically the same experiences of doubt, depression, elation, religious experience, etc. were indicated, and the causal influences mentioned were also nearly the same, differing only in minor details. This suggested to me that the answers to the detailed experiences called for under Topics II, III and IV were more reliable than the classification of those experiences called for by Topic I. This proved valuable, for it justified me in a few instances in placing the respondent in a different class from the one he had himself indicated. In a few returns the answers under the later topics clearly showed that the wrong class was checked under the first topic. Wherever possible, however, I consulted the person that supplied the data before making a change. These consultations confirmed the suggestion that the answers to the later topics were more reliable than those to the first.

Under the space for the respondent's signature were placed these words: "If signing would keep you from answering frankly, do not sign." Eight and one-half per cent. of the men and 14 per cent. of the women did not sign. Most of these, however, were members of a large class in which the instructor called especial attention to the fact that the signature would not be necessary. There was very little spontaneous tendency to withhold the signature.

PSYCHOLOGICAL LITERATURE.

The Vocation of Man, by JOHANN GOTTLIEB FICHTE. Translated by William Smith, with biographical introduction by E. Ritchie. Chicago, Open Court Publishing Company, 1906. pp. lxii + 178.

Spinoza and Religion, by ELMER ELLSWORTH POWELL. Chicago, Open Court Publishing Company, 1906. pp. xiii + 344.

An Introduction to Philosophy, by GEORGE STUART FULLERTON. New York, Macmillan Company, 1906. pp. xiii + 322.

Concepts of Philosophy, by ALEXANDER THOMAS ORMOND. New York, Macmillan Company, 1906. pp. xxvii + 732.

An Outline of the Idealistic Construction of Experience, by J. B. BAILLIE. London, Macmillan and Co., 1906. pp. xx + 344.

Everyday Ethics, by ELLA LYMAN CABOT. . New York, Henry Holt and Co., 1906. pp. xiii + 439.

Few works by any philosophical writer are more stimulating than Fichte's "Vocation of Man," which keeps ever before the reader the intimate connection of abstract speculation with the profoundest problems of human life; no work of its author exhibits so clearly the spirit and substance of his system. The Open Court Publishing Company renders the student a distinct service in adding this standard translation of it to its series of cheap reprints of philosophical classics.

In "Spinoza and Religion," Professor Powell, of Miami University, examines a question of perennial interest to students, and one still perhaps unsettled. Was Spinoza, as his contemporaries generally judged him, the 'prince of atheists,' or was he, as he has frequently since been regarded, a 'God-intoxicated' mystic? Taking the view that religion essentially involves belief in a higher power, or powers, of a personal kind, and that Spinoza's 'God' is plainly impersonal, the author concludes that Spinoza's system really is, and was intended to be, atheistic and anti-religious. The question turns chiefly on the interpretation of such technical terms as *intellectus infinitus*, *cogitatio infinita*, and *idea Dei*, and on the interpretation of certain apparently contradictory passages, such as Eth. II, 3, where it is said that "in God there is necessarily an idea as well of his essence as of all things that necessarily result from his essence," and Eth. I, 17, Schol., where an intellect and will like ours are expressly denied to God. Professor Powell shows considerable skill in dealing with the difficulties raised by the ambiguities in Spinoza's language, his great principle in explaining away the apparent implications of consciousness in God in such passages as the first here referred to being that of accommodation to scholastic modes of expression. He carries this principle so far, however, as to make it difficult to exonerate Spinoza from the charge of deliberate deception. If Spinoza was no mystic, his system is certainly a stupendous mystification. But if, as the author has so well shown, Spinoza's thought is full of contradictions, why not include his theism and mysticism with the rest?

Opinions naturally differ as to the best method of introducing college students to the study of philosophy. In a discipline which depends so much for its success on the personality of the teacher as well as on the character of the student and the conditions of the curriculum, there is probably no 'best' method. In the view of the present writer, there is no better approach to the problems of reflective thinking than through a sympathetic study of the history of Greek philosophy. It is impossible, however, to read Professor Fullerton's excellent "Introduction to Philosophy" without recognizing the many advantages of his method of approach, especially when used as a supplement to the historical. The arrangement of topics—the general nature of philosophy, the problems of the external world, the problems of the mind, the various types of philosophical theory, etc. —is admirable, and the distribution of the material well balanced. Particularly to be remarked is the skillful way in which Professor Fullerton, starting with the assumptions of the plain man, leads the student into the intricacies of the problems as they appear on mature reflection. His own views are expounded and defended with energy and exemplary clearness, and many of these the teacher of the book is likely as vigorously to combat; but the significant thing is that these views are presented with a singular freedom from dogmatism and that the student is everywhere stimulated and encouraged to think for himself. The book is the ripe product of many years of successful teaching, and for purposes of instruction is easily among the very best of its class.

From an introduction for beginners we pass in Ormond's "Concepts of Philosophy" to an elaborate treatise capable of taxing the patience and thought of the most advanced philosophical student. The work shows the same high qualities of grasp and penetration, of originality and conservatism, as the author's earlier volume, "The Foundations of Knowledge;" here, as there, the thought is always independent, thorough and painstaking, moving leisurely and untrammelled in wide open spaces. Ormond's point of departure is the opposition of science with its mechanical, and metaphysics with its teleological categories. Beginning with an analysis of the most general conceptions underlying respectively the scientific and philosophical constructions of experience, he then proceeds synthetically, by a successive advance from the physical to the social and thence to religion, to lay the foundations for a final, unified interpretation. He seeks to show that the concepts and methods of physical science, while applicable throughout the whole range of experience, are inadequate to the demands of synthesis which arise, with increasing urgency, as we ascend the scale of the sciences, and that they point to their transcendence in metaphysics, which subordinates the mechanical to the teleological, and in religion, in which the unity required is complete. By this method a system is constructed which carries to its conclusion the 'Copernican revolution' initiated by Kant, the central contention being that the fundamental reality is 'consciousness,' the energy which is aware both of itself and of its object; ultimately, the appeal is to an 'eternal consciousness.' But the unification of truth demanded by philosophy requires, it is held, not only the synthesis of science and metaphysics, but also the synthesis of knowledge and belief, and this final synthesis is an act of will. Hence God, freedom and immortality, to which theoretical considerations 'point,' appear, in the end, as practical postulates.

The idealistic construction of experience is carried out in a still more rigorous fashion by Professor J. B. Baillie, of the University of Aberdeen. His method is the method of Hegel, and his argument a free reproduction of the argument of the 'Phenomenology of Spirit.'

He seeks to show (1) that each phase of experience embodies in a specific way the one spiritual principle that animates all; (2) that each is distinct from the other simply by the way it embodies this principle; (3) that each is related to the others and to the whole in virtue of its realizing the principle with a certain degree of completeness; (4) that the whole of experience is a necessary evolution of the one principle through various forms logically connected as a series manifesting the principle. The main stages of the development are, first, that in which the individual subject is conscious of objects as *prima facie* outside the subject; second, that in which it is conscious of self as other than, and yet implicitly one with the subject; and third, that in which all sense of otherness is overcome and self and subject are transparently one (p. 134). No instructed reader can well deny the force and fascination of the method. It 'adopts,' if one chooses to say so, the principle it seeks to demonstrate, but it does this in no arbitrary way, for the principle is not external, one to which experience has to be fitted, but immanent; experience, in other words, essentially involves some sort of unity and relation of subject and object. The difficulty lies in connecting its various phases without mutilating any one of them, and in showing that the postulated unity is a really constitutive, and not merely an ideal or regulative principle of the whole. Professor Baillie has done, perhaps, all that can reasonably be expected to make the principle clear. The pragmatist who regards the unity of experience as confined to the individual experience from moment to moment will not be convinced, but he can hardly afford to ignore Professor Baillie's sympathetic, but penetrating, criticism (pp. 10-25).

Mrs. Cabot's instructive and well-written work on "Everyday Ethics" may be heartily commended to teachers in secondary schools who are looking for an experienced guide in the development of moral consciousness in their pupils. The book is the outcome of years of experience with the minds and needs of just such pupils. It avoids technical and abstract discussions and deals, in a live way, with the problems in which pupils of the high school age are, or can readily be made, interested. Its central ethical doctrine is that duty is found in fidelity and efficiency in one's chosen vocation, that "out of loyalty to our chosen work springs all moral life." Added to the main body of the text is a key to teachers, containing many excellent suggestions, numerous questions and additional illustrations.

Smith College. H. N. GARDINER.

The Elements of Psychology, by EDWARD L. THORNDIKE. A. G. Seiler, New York, 1905. pp. xix, 351.

An Introduction to the Theory of Mental and Social Measurements, by EDWARD L. THORNDIKE. The Science Press, New York, 1904. pp. xii, 212.

The aim of *The Elements of Psychology* is "to help students to learn the general principles of psychology." The volume is "designed to serve as a text-book for students who have had no previous training in psychology, who will not in nine cases out of ten take any considerable amount of advanced work in psychology, and who need psychological knowledge and insight to fit them to study, not the special theories of philosophy, but the general facts of human nature." The book is divided into three parts (Descriptive Psychology, Physiological Basis of Mental Life, and Dynamic Psychology), an Introduction and a Conclusion.

The Descriptive Psychology (Part I) is a modified abstract of James's *The Principles of Psychology*. It gives a general account of 'mental states,' 'thoughts,' and 'feelings,' with a final chapter on the 'functions of mental states.'

Part II is, with the exception of chapter xi, neurological. It gives, in terms of the neurone theory, the principal structural and functional characteristics of the central nervous system and of the sense organs. This section of the book is abundantly (even superfluously) illustrated from standard neurological texts.

Dynamic Psychology (Part III) is defined as the "science of the mind in action." It has to do with "the facts and laws which determine what any human being will think and feel and do, how he will learn, why he will misunderstand, when he will be interested, what habits he will form, to what sort of intellect and character he will attain." This 'science,' as it appears in the book under consideration, shows the impress of modern biology, both of the descriptive and of the statistical type. Its emphasis falls upon 'instincts' and 'capacities,' 'native and acquired tendencies,' 'inborn connections,' and 'situations.'

It is clear that dynamic psychology is one form of the psychology of reactionism. Its interest in consciousness may almost be said to be incidental. Dynamic psychology is the psychology of 'behavior,' of 'conduct,' of 'human life.' It is concerned with consciousness only in so far as consciousness is linked to stimulus and to organic movement; only in so far as it is a factor in adaptation. It studies the 'function' of thoughts and feelings, and "the function of thoughts and feelings . . . . is to influence actions" (*i. e.*, movements). Emphasis is accordingly laid upon three sets of connections: connections between stimulus and consciousness, connections between one thought or feeling and another, and connections between consciousness and organic movement: in the author's terms, connections of impression, association and expression. Each of these may be further divided into original (unlearned) and acquired (learned) connections or tendencies to connection (six sets in all).

Now, by a liberal interpretation of the venerable principle of associative relationships, dynamic psychology proceeds to state, in the form of 'laws,' the conditions under which the six kinds of connection are realized; *i. e.*, the way in which (1) stimulus leads (under native and acquired tendencies) to consciousness, (2) consciousness to consciousness, and (3) consciousness to movement.

Since it is in these 'laws' that Professor Thorndike's dynamic psychology comes to its chief issue, it is perhaps worth while to inquire whether the formulations in question really preserve (in spite of the generous use of capital letters) the dignity and the significance of the scientific law. They are in reality rules;—not laws, either in the sense that they state invariable connections of antecedent and consequent or as predictive anticipations of fact. As descriptive expressions of the dependence of a phenomenon upon an indefinite number of heterogeneous factors whose values are uncalculated and whose modes of combination are unknown, these 'laws' (especially the "entire Law of Association") are the counterpart of the glittering generality of common sense. Formulations of this kind are impressive, and their indeterminate nature saves them from refutation; but their usefulness to science is uncertain and their effect upon "the naïve student" unfortunate.

Even if the formulas in question were proper psychological laws, the still more fundamental objection might be raised that they are, from the point of view of mind, external and superficial. They refer either to the organic conditions and consequences of consciousness or to the 'secondary' factors in the successive or linked association. This crude use of association in psychology is surely obsolescent and should be obsolete. The more refined analytic methods long ago discarded it. The biological rehabilitation of the text obscures but does not conceal the stock principle of associationism.

I have dwelt upon the author's associative connections both because they furnish the basis and superstructure of the Dynamic Psychology and because they illustrate at once the weakness and the strength of the point of view. The author's method is the method of gross anatomy; the method that regards consciousness as a whole, that calls it a stream, notes its rate and volume, its fascinating evolutions and metamorphoses, its multifold relations to objects, to the processes of knowledge, to conduct, to the physical organism; and that contents itself, on the side of analysis, by distant reference to 'feelings' and 'thoughts.' Whenever it comes to cross-section, to careful scrutiny of the snap-shot picture of mind, to the histological analysis of the individual thought or feeling, there—unless its champion happens to have a gift for introspective subtleties—the method balks. Consider the work under review. Problems of the first sort are treated with competence; problems calling for histological treatment are either avoided or drawn in caricature. Take the subject 'sensation.' The sensation of the laboratory is scarcely mentioned, and the author, realizing that "definitions must be rough" (and apparently they *may* be, since the reader is the "naïve student"), actually confuses the sensation-element with Fechner's sensation-magnitude and with Münsterberg's sensation-atom! Similarly, stimulus is defined in two inconsistent ways (17 and 28); and fusion and colligation are gravely said (in a 14-line account of "the constitution of percepts") to be combinations of "brain processes." Again, it is, in the reviewer's opinion, doubtful whether a student who had been 'introduced' to psychology through the *Elements* could, on occasion, give an intelligible definition of the terms 'mental state,' 'thought' and 'feeling;'—could tell whether the terms were synonymous, whether a mental state is a group of feelings or thoughts, or whether a thought is a particular kind of feeling or mental state. Action, finally, offers a glaring instance of the lack of analytic insight into the constitution of mind. The dynamics of action is chiefly interested in the way in which feelings lead up to, and issue in, movement. The very term 'action' has entirely lost its psychological meaning and implies simply muscular contraction and its bodily results. The whole problem of action, considered as a matter of consciousness, is missed, or, at most, it emerges in such bootless form as the contention that movement images are rare antecedents of voluntary action.

When an author has succeeded in writing a text-book that is at once interesting and impressive, a book of high pedagogical merit and of clever arrangement of fact and principle, of instance and illustration, detailed criticism, especially of the negative sort, is apt to appear captious if not carping. Apart, however, from the demands of the science, the critic finds, in a case like the present, ample justification for his strictures—if nowhere else—in "the right of the student . . . to demand a fair representation of the science as a whole" (preface, page x).

In his book on the Theory of Mental and Social Measurements, Professor Thorndike "has had in mind the needs of students of economics, sociology and education, possibly even more than those of students of psychology, pure and simple." The book presents in an interesting and simple, if not systematic, way the principles and methods employed in the modern science of biometry. The present writer has found the volume serviceable for occasional reference and for the collateral use of students, both in psychophysics and in the statistical study of psychological problems. It contains, in an appendix, convenient tables for arithmetical computations.

Cornell University. I. MADISON BENTLEY.

Skalpieren und ähnliche Kriegsgebräuche in Amerika, von GEORG FRIEDERICI. Vierweg, with map. Braunschweig, 1906. pp. 170.

In this monograph of characteristic German exhaustiveness, the author discusses the etymology of the word, scalping, and gives its history from Herodotus to the present time. Although it did occur in antiquity, it seems to have completely vanished in Europe, while head trophies or sometimes other parts of the body that were cut off, took its place as evidences of victory in war. Scalping is essentially a characteristic trophy of the New World, and in an interesting colored map the author shows how it probably originated among the Indians of the eastern part of America although it had probably an independent origin in a much smaller area in Central South Africa. In America, it spread westward from the region of the Great Lakes, north to Hudson Bay and west to the Rocky Mountains and south to Mexico and later nearly to the Pacific Coast. Over nearly all of Africa, in Central Mexico and the Pacific Coast, it occurred occasionally, although in this latter region heads were often used as trophies.

Folkways, a study of the sociological importance of usages, manners, customs, mores and morals, by WILLIAM GRAHAM SUMNER. Ginn & Company, Boston, 1907. pp. 692.

This book, in both its plan and presentation, reflects the vigorous and unique personality and the power of original thought of its author. He first characterizes *mores*, then the struggle for existence, labor, wealth, societal selection, slavery, abortion, infanticide, killing of the old, cannibalism, then passes to sexual *mores*, and marriage institutions, social codes, incest, kinship, blood revenge, primitive justice, peace unions, uncleanness and the evil eye, points out how *mores* can make anything right and prevent condemnation for anything. In illustration of this, he characterizes sacral harlotry and child sacrifices, then passes to consider popular sports, exhibitions, drama, asceticism, education, history, life, policy and virtue versus success. He uses the Latin word *mores* to designate popular usages and traditions when they include a judgment that they are conducive to societal welfare and exert a coercion upon the individual to conform to them, although they are not co-ordinated by any authority. He has also sought to bring the words *ethos* and *ethology* again into familiar usage. After analyzing folk ways, he attempts to justify this process by a series of illustrations, and opines that this in order to be successful must go into details. These of course are immense, so that he can only select those deemed most fit from a larger array of facts which were used in forming his generalizations. Indeed, the original plan of the book has been curtailed, for he intended to include demonism, primitive religion and witchcraft, the status of women, evolution and the *mores*, usury, gambling, societal organizations and classes, mortuary usages, oaths, taboos, ethics, æsthetics and democracy. The first four of these we are glad to know are already written. The index is deserving of special commendation.

The Káfirs of the Hindu-Kush, by SIR GEORGE SCOTT ROBERTSON. Illustrated by A. D. McCormick. Lawrence & Bullen, London, 1900. pp. 658.

This new edition has been re-edited and reconstructed as indeed historic needs made necessary, if it was to be brought to date. Since the first edition appeared in 1897, the whole of Kafiristan has been consolidated. Moslem missionaries have been massacred, and sometimes barbarous executions are thought to be more deterrent to violent and predatory crimes than the endless hangings and life imprisonment which characterize the British government of India. Moslem missionaries do not worry nor exasperate, but take plastic boys to Kabul and make them zealous followers of the great prophet of Arabia. The fervor of proselytes is proverbially without limit. Whatever the future of this land, it will not change its religion, for here the Kafirs have become Sheikhs. In emergencies the old heathen rites will crop up again, but the country is now Mohammedan.

Weird Tales from Northern Seas from the Danish of Jonas Lie, by R. NISBET BAIN. With twelve illustrations by Laurence Housman. Kegan Paul, Trench, Trübner & Co., Ltd., London, 1893. pp. 201.

This book consists of eleven tales translated from the Danish of Jonas Lie whose work has long been well known to all interested in such themes. He is best where he tells the weird legends of his native province, Nordland, in which he was himself brought up. The folklore of these lonely, subarctic tracts is in keeping with the savagery of nature. Elves and gnomes are rarely friendly, but all the supernatural beings that haunt sea and shore in these regions are malignant, malific, hating man and delighting to mock his toil and sport with his despair.

Hinter Kerkermauern; Autobiographien und Selbstbekentnisse, Aufsätze und Gedichte von Verbrechern; ein Beitrag zur Kriminalpsychologie, von JOHANNES JAEGER, Mecklenburg, Berlin, 1906. pp. 436.

We have here an interesting and unique volume made up almost entirely of writings of prisoners. They are classified first, as autobiographic or confessional; second, those describing the cause of the crime, or the criminal propensity; third, those expressing the meditations and reflections of criminals in prison; fourth, religious thoughts; fifth, the opinions of criminals on social questions; sixth, their views on penology. Many poems are included. The author is a prison chaplain who appears very little, but expresses his most emphatic dissent from the conclusions of the Lombroso school. He even denies that there are any typical varieties of criminals or that there are morphological or psychological traits, but thinks that crime is essentially a product of the *milieu*. Criminals, he insists, show the same psychological traits that others would show under like conditions. This opinion he bases upon fifteen years experience with them. These psychological documents certainly give a most interesting inner view of the souls of a class of people, knowledge of whom is commonly a book of seven seals.

Woman, by BERNARD S. TALMEY. The Stanley Press, Chicago, 1906. pp. 228.

This book is written by a gynecologist and is designed for physicians and students of medicine. In the introduction, we have thirteen brief chapters of phallicism, prudery, results of silence, love and passion, etc. Then follows the evolution of sex, a chapter each upon anatomy and the physiology of the sexual instinct and act. Part fifth is devoted to pathology and then follow three larger parts on hygiene, psychology, and morality respectively. The book is written with extraordinary plainness and with little attempt to beat the bush in discussing delicate matters.

Man and Woman, the female the higher type, by WILLIAM T. BELFIELD. E. W. Broman, Chicago, 1907. pp. 111.

Mr. Belfield is well known as a writer upon the pedigree and heredity of the horse and to this he has contributed many important and valuable points. In this pamphlet he turns his attention to stirpiculture in the human field and discusses briefly and aphoristically such topics as puberty, senility, descent, the comparison of the sexes, nameless diseases, etc., with particular reference to their evolutionary bearings.

The Inter-generation Period, by CHARLES H. CHANDLER. From the Wisconsin Academy of Sciences, Arts and Letters. Vol. XII, pp. 499-504. Issued in October, 1899, in advance of the general publication.

The author examined nearly 16,000 dates of births of New England families and finds that on the average, the period intervening between successive generations is 33⅓ years. He concludes, after a summary, that the more nearly complete the record of births in each generation and the greater the number of generations included in the examination, apparently the greater is the tendency to a mean period of one-third of a century. The very ingenious graphic method represented in the diagrams is of itself interesting. The factors which lengthen the inter-generation are large families and marriages in which the woman is much younger than the man.

A Study of Longevity, by CHARLES K. CHANDLER. Reprinted from the Transactions of the Wisconsin Academy of Sciences, Arts and Letters. Vol. XIV, Part I. Issued September, 1903, in advance of general publication. pp. 56-62.

The writer has based his work upon the records of eight families extending back to the beginning of the seventeenth century and containing more than 100,000 names. From this it appears that there has been little if any gain in respect to the proportion of deaths among young children. This the writer ascribes to the increase of the perils of infant life, due to growing urban population which balances all hygienic advance. The median age seems, however, to be advancing, which indicates a decrease of deaths in youth and early manhood. This permits the pessimistic view that our advance in overcoming disease is not wholly a blessing because many who by the beneficent working of the law of survival of the fittest would have been removed early in life are now preserved to become the progenitors of feeble offspring. There is a marked decrease in the proportion of people attaining extreme age, despite the increase of the average length of life.

The Roots of Reality, being suggestions for a philosophical reconstruction, by ERNEST BELFORT BAX. E. Grant Richards, London, 1907. pp. 331.

Starting from certain postulates united on the consistency of consciousness, the author attempts to rough hew certain indications of the direction in which future philosophic constructions must take place if they are to be even relatively adequate to the needs of the up-to-date philosophic mind. The author is frankly and fully an idealist, although not perhaps in the form in which idealism has loomed largest for modern thought, but he seeks a more adequate formulation. The chief topics are the general problem of conscious idealism, the illogical and the logical as the ultimate elements, the individual consciousness, reality, true and higher consciousness, the final goal of all things and the problems of metaphysics. The author is loyal to, and has a subdominant interest throughout in, sociological problems.

Philosophie der Unbelebten Materie; hypothetische Darstellung der Einheit des Stoffes und seines Bewegungsgesetzes, von ADOLF STÖHR. Mit 35 Figuren. Barth, Leipzig, 1907. pp. 418.

In 1904, the author published a volume entitled Zur Philosophie des Uratomes, in which he developed the view that physical processes could be sketched between the two extremes of mono- and poly-energetics. Since then, much rather revolutionary experimental work has been done and a very stimulating theoretical construction published by Sahulka, which attempts to explain gravity, molecular energy, heat, light and electricity in a purely mechanical and atomistic way. Stöhr differs from Sahulka in assuming that the impenetrability of atoms is a problem, not a certainty, and secondly, he has a very different formula for the relation of two atoms in action upon each other. In general, inanimate matter is an aggregate, built up of elements of from one to seven orders, with various vibrations, times, rotations, etc. As a whole his conclusion hardly may be said to take too little cognizance of the recent excellent work done on ions.

The Persistent Problems of Philosophy. An introduction to metaphysics through the study of modern systems, by MARY WHITON CALKINS. Macmillan & Company, New York, 1907. pp. 575.

This book, the author frankly tells us, was not written to lure unphilosophical students into the field, although designed for beginners, but is an attempt to combine the essential features of a systematic introduction to metaphysics with those of a history of modern philosophy. The chapters are as follows: The nature, types and value of philosophy; Systems of numerical pluralism; Pluralistic materialism, the system of Hobbes; Pluralistic spiritualism, the system of Leibnitz; Pluralistic spiritualization, the system of Berkeley; Pluralistic phenomenalistic idealism, the system of Hume; An attack upon dualism and phenomenalism, the critical philosophy of Kant; Monistic pluralism, the system of Spinoza; The advance toward monistic spiritualism, the systems of Fichte, Schelling, and Schopenhauer; Monistic spiritualism, the system of Hegel; Contemporary philosophic systems, the issue between pluralistic and monistic personalism. Appendix consisting of biographies and bibliography of the modern rise of philosophy, together with summaries and discussions of certain texts.

Pragmatism, a new name for some old ways of thinking. Popular lectures on philosophy, by WILLIAM JAMES. Longmans, Green & Company, New York, 1907. pp. 309.

We have at last in this volume Professor James's exposition of pragmatism. He discusses what it means, the present dilemma in philosophy, some metaphysical problems pragmatically considered, the one and the many, pragmatism and common sense, pragmatism's conception of truth and relations to humanism and to religion. The form of the book is eminently popular and this affords the author an opportunity to bring to bear his remarkable and charming style which is always engaging and captivating. A fuller review will follow.

The Philosophy of Goethe's Faust, by THOMAS DAVIDSON. Edited by Charles M. Bakewell. Ginn & Company, Boston, 1906. pp. 158.

Though dead, he yet speaketh. These six lectures were given in the winter of 1896 and in them Mr. Davidson told what Faust, whom he knew almost by heart, had come to mean for him. He believed that true poetry might include all the content of philosophy and much of that of religion presented always in concrete form. To him the content of Faust meant the entire spiritual movement toward individual emancipation composed of the Teutonic Reformation and the Italian Renaissance in all their history, scope and consequences.

In the Fire of the Heart, by RALPH WALDO TRINE. McClure, Phillips & Co. New York, 1906. pp. 336.

This book seems to be written in the author's best style and tells us how time deals with nations, describes governments, great place movements, public utilities, labor, the agencies for attaining the greatest good, and finally the best chapter of all, the life of the higher ideals and power.

Das Wesen des Menschlichen Seelen- und Geisteslebens, als Grundriss einer Philosophie des Denkens, von BERTHOLD KERN. Hirschwald, Berlin, 1907. pp. 434.

This physician develops a somewhat unique system of philosophy in eight chapters which discuss the following topics. The problem of the soul and the methods of studying it; the foundation and the essence of knowledge; the life of the will; the identity of the soul and body; logical thought and knowledge; noetic thought, life and development; the unity of the psycho-spiritual processes of sensation, feeling, will and thought; spiritual freedom and ethics.

Der Mechanismus des Denkens, von HENDRICK DE VRIES. Mit 5 Textabbildungen. Hager, 1907. pp. 64.

After twenty years of careful study and reflection, the writer reached the conclusion that the essence of thought can be understood, and the old idea that we never can know what it is, is all wrong. Beginning with memory, the writer gives us a diagram of how he conceives it to have been represented in the mechanism of the optic lobes. He then describes the nature of the simple and the complex idea and then comes to the will, the processes of which he represents in a very complex and yet intelligible series of diagrams. Consciousness is evolved from speech and his theory on this topic is expanded into great complexity, taking its point of departure from the current schematisms concerning aphasia. In the supplement, he discusses the centre of the first and second optic word image. Whatever else may be said of his conclusions, a few of them are certainly most stimulating, original and ingenious. Of course, in presenting such views, the writer goes far beyond the standpoint to which he is logically compelled by facts.

Social and Ethical Interpretations in Mental Development, a study of social psychology. By JAMES MARK BALDWIN. 4th ed. The Macmillan Co., New York, 1906. pp. 606.

This fourth edition is not materially altered, the changes being chiefly additions of literary references and notes. The third edition brought the work into practically its final shape. The author's genetic logic is closely related to this book in that it seeks to trace the meaning of consciousness. The result is summed up in the phrase "the individual is a social outcome in a social unit and knowledge is common property and not a private possession." This thesis to the author's mind destroys the epistemological atomism and subjectivism of individualistic theories of knowledge making personal logical thought an outcome and not a unit, just as the first of these truths destroys individualistic theories of state and society.

Growth and Education, by JOHN MASON TYLER. Houghton, Mifflin & Company, Boston, 1907. pp. 294.

The author has collected here much matter concerning the growth of different organs and systems which are scattered through medical journals or are published in separate monographs. He assumes growth to be more important than learning. Balance of organs is important, and right physical, mental and moral habits. The author is a great believer in the educational value of the old New England farm. A general idea of the scope of this book may be presented by the topics of the chapters which are as follows: Present needs in Education, Man in the Light of Evolution, Hints from Embryology, Growth in Weight and Height in the Neuro-muscular System and the Visceral Organs, Mortality and Morbidity, Constitution and Periods of Life, The First Three Years, The Kindergarten Period, In the Grammar Grades, In the High School, Physical Training, Play and Gymnastics, Manual Training. As a whole, the book is a rare and very valuable combination of the results of science and of robust common sense. It is a book which ought to be accessible to every teacher of all grades. The title of it gives only a faint indication of its range and scope.

Vybrané stati pedopsychologické a pedagogické. G. S. HALL. Se svolením auktorvym prelozil Mauer. úvodem provázi. Prof. Dr. Frant. Cáda. Praze, 1906. pp. 199.

Selected paido-psychological papers, translated into Czech with permission of the author (Dr. G. Stanley Hall). Introduction by Dr. Frantisek Cada. Prague, 1906. Contents:

Introduction, Dr. G. Stanley Hall and His Activities. 1. Notes on the Study of Infants. (From *Ped. Sem.*, June, 1901, Vol. 1, pp. 127-138.) 2. Some Aspects of the Early Sense of Self. (From *Am. Jour. of Psy.*, April, 1898, Vol. 9, pp. 351-359.) 3. Contents of Children's Minds on Entering School. (From *Ped. Sem.*, June, 1891, Vol. 1, pp. 139-173.) 4. Moral and Religious Training of Children. (From Princeton Rev., Jan., 1882, Vol. 10, pp. 26-48.) 5. The Love and Study of Nature a Part of Education. (From Rep. of State Board of Ag. of Mass., 1898, pp. 134-154.) 6. Children's Lies. (From *Ped. Sem.*, June, 1891, Vol. 1, pp, 211-218.) 7. The Ideal School as Based on Child Study. (From Forum, Sept., 1901, Vol. 32, pp. 24-29.)

This volume consists of a translation and epitome of seven of the psycho-pedagogic papers of G. Stanley Hall which have been translated into Bohemian by Professor Dr. F. Cáda. The whole is prefaced by a sketch of the life of Dr. Hall. This much we gather from a kind note of Dr. Cáda, but we are unable to say more of the book.

Symmetrie und Gleichgewicht. Ausstellung in Königl. Württ. Landesgewerbemuseum. Stuttgart, 1906. Katalog, im Auftrage der Königl. Zentralstelle für Gewerbe und Handel, von GUSTAV E. PAZAUREK. Stuttgart, 1906. pp. 160+16.

This unique little book first discusses the etymology and the idea of symmetry and equilibrium in art. The author then shows their precursive forms in nature and then passes to some details of symmetry in dress, arms, heraldry, art, handiwork, and shows the different treatment of symmetry in the conservative and oppositional styles. A number of illustrations show the extremes both of symmetry and of glaring departures from it. The book as a whole is designed as a supplemental guide-book through the royal exposition of the museum in Stuttgart.

Handbuch der Kunstgeschichte von ANTON SPRINGER. V. Das 19. Jahrhundert, Dritte Auflage, bearbeitet und ergänzt von Max Osborn. E. A. Seeman, Leipzig, 1900. pp. 452.

This new volume of the third edition with its 490 cuts and twenty-three colored plates constitutes one of the best of the series which as a whole is distinctly the best we have in its field. To set forth the history of art in recent years is a tremendous task, especially if one require himself to select representative pictures of all schools and lands which shall illustrate the chief and typical tendencies. While we cannot speak with the authority of expertness upon this matter, the fame which this series has already attained and the high quality make it probable that it will not soon be superseded.

Psychology, general introduction, by CHARLES HUBBARD JUDD. Vol. I of a series of text-books designed to introduce the student to the methods and practice of scientific psychology. Charles Scribner's Sons, New York, 1907. pp. 389.

The author has adopted what he calls the genetic method, which, as he uses it, is very different from the evolutionary method. Functional seems to us a better term. The physiological conditions of mental life have been given a conspicuous place and much attention is given to ideation. The writer first treats the evolution of the nervous system, then that of man and then passes to a general analysis of consciousness, treating each of the five senses and their relations to space, time and unity. He then discusses stress and expression, instinct and habit, memory and ideas, language, imagination and the formation of concepts, concept of self, impulse and voluntary choice, forms of dissociation and practical applications. The book has 56 illustrations, the most of which, however, are not new. To our thinking, it is in general too abstract for a beginner's book. While there are many, even a great many illustrations, the author's *tendence* is too much in evidence. Nevertheless it should be welcomed, and certainly presents many things not found in other texts.

L'Année Psychologique, publiée par ALFRED BINET. Masson et Cie, Paris, 1907. pp. 495.

This volume of l'Année contains an unusual number of original memoirs. Some of the most important of these are on the relativity of space, the progress of psychophysics, perception of psychic facts, the relations between insects and the colors of flowers, work of Pawlor on the secretion of the so-called psychic saliva, relation between medicine and pedagogy, and between psychology and metaphysics, between touch and muscular sense, visual memory of normal and abnormal children, muscular and articular sensibility, acquisition of habits by animals, the writing experts in the Dreyfus trial, Weismann's theory of the genesis of instinct, scientific study and experimental work, mental enfeeblement in dementia praecox, senile dementia and general paralysis, chronic mental confusion, the question of race in psychology, the psycho-chemic conditions of the functioning of nervous centres, the co-operation of the school and the family, the development of the problem of aphasia, pain and pain nerves, sensitive tracts in the nervous system, sexual dimorphism in plants, the modern doctrines of morals and the psychology of thought.

The Psychological Clinic, edited by Lightner Witmer. The Psychological Clinic Press, Philadelphia, March 15, 1907. pp. 40. Vol. I, No 1.

This is a new journal for the study and treatment of mental retardation and deviation, publishing nine numbers a year; each of 28 or more pages at a subscription of $1.00 per annum. It is published primarily in the interests of retarded and defective children and will take cognizance of all forms of such work for mentally and physically abnormal children and youth and juvenile delinquents and dependents, including the blind and deaf. It will appeal to those having philanthropic interests. Professor Witmer has for ten years conducted a psychological clinic. The purpose of the movement which his work represents is fourfold; the investigation of the phenomena of mental development of school children, especially those who are retarded, and (2) a clinic supplemented by a training school at the hospital, (3) the offering of practical work to those interested alike in teaching, medicine, social observation and training, and (4) the training of students for a new profession, namely that of the psychological expert who shall make his career in connection with the school system. The first number makes an attractive appearance, an interesting case of infantile stammering in a boy of twelve is well studied, a principal of a Philadelphia school describes a case of juvenile delinquency, the editor has a long article on university courses in psychology in general, but with special reference to this kind of work, while there is another department of book reviews, criticisms, notes, news and comments.

The Philippine Journal of Science, edited by PAUL C. FREER. Published by The Bureau of Science of the Government of the Philippine Islands. Manila, October, 1906. Vol. I, No. 8. pp. 791-08.

The Philippine Journal of Science, edited by Paul C. Freer, is an important part, one might almost say, of the Philippine question in this country. Some of its work is excellently done. It is generally rather more practical than scientific in its range and scope. It is plain, however, that the purpose of the editors is not limited to the utilitarian side, but the anthropology, folklore, flora, fauna, geology, rainfall, etc., of the islands are included.

On the Functions of the Cerebrum; the frontal lobes, by SHEPHERD IVORY FRANZ. Archives of Psychology. Edited by R. S. Woodworth. No. 2. March, 1907. New York, The Science Press. pp. 64.

The author first gives some account of the frontal lobes as centres of motion, inhibition, attention and intellectual states, then describes his own method and his results, which show loss of habit after extirpation of the frontals, the effect of surgical shock upon the attention, associations, the retention of habit after extirpation of parts of the cerebrum, the formation of association after removal of both frontal lobes, emotion, will, condition of animals from which the lobes have been removed and the nutrition.

THE PROCEEDINGS OF THE PHILADELPHIA MEETING OF EXPERIMENTAL PSYCHOLOGISTS.

The fourth annual gathering of experimental psychologists was held at the University of Pennsylvania on April 17th and 18th, 1907. The papers were of a high degree of excellence, and the discussions which followed were interesting and illuminating. A novel feature of the programme was a round table discussion of the plans and methods of instruction in psychology. The visiting psychologists were invited to attend the meetings of the American Philosophical Society which were held in Philadelphia during the same week. Professor Witmer entertained the psychologists most hospitably at the University Club, and conducted them on tours of inspection through his unusually well-appointed laboratory.

In the session devoted to Comparative and Child Psychology the following papers were presented:

Statistics of Retardation in a City School System, by J. E. BRYAN.

A statistical study of data obtained from several thousand children of all school-ages and school-grades. There comes to light a thoroughgoing parallelism between physical anomaly and mental retardation. (This paper has since appeared in full in the *Psychological Clinic.*)

Clinical Examination of Retarded Children, by J. D. HEILMAN.

A description of the mode of procedure followed in the examination of retarded children, and a presentation of certain results obtained, with illustrations from individual cases. The procedure consisted essentially in obtaining from the school records a list of pedagogically retarded children, and the preparation of a blank record-sheet for the systematic enumeration of data. In addition to the mental, physical and motor stigmata, these sheets recorded such data as home and family conditions, and school standing. The individual cases presented showed that the capacity of pedagogically retarded children may vary between the limits of good mentality and hopeless idiocy.

(1.) *Individual Variation in Acquisition and in Retention*, by E. J. MYER, and (2) *The Associative Processes of the Dog, the Cat and the Squirrel*, by S. S. COLVIN. (Presented by J. W. Baird.)

(1) Mr. Myer's investigation dealt with the differentiation of the memorial types in school children, and the relative memorial efficiency of the various types. His preliminary work had to do chiefly with a series of tests which should identify the type present in a given individual. This was followed by a series of experiments which aimed to determine the stability and the relative efficiency of the different types. In this preliminary report the tests employed were described and demonstrated. (2) Professor Colvin's research aimed to obtain quantitative determinations of the ability of certain animals to distinguish colors, and to establish associations between groups of memorial material. A series of boxes of uniform size and shape, but of different colors, was prepared and furnished with food. Only one of the boxes,—the standard,—could be opened by the animals. After a preliminary series of experiments during which the animals learned to select and open the standard box, this box was exposed in various combinations with the others, and the reactions of the animals,—dogs, cats and a squirrel,—were noted. It was found that all of the animals were able to distinguish between the six more prominent colors of the spectrum, and even between but slightly different tones of the same color. No color showed any pronounced superiority over any other. The results also show that the animals tested possess a rapid acquisition, and a fairly tenacious memory. (These two papers will appear in full in a forthcoming volume of the *Psychological Review* Monograph Series.)

In the session for Laboratory Methods of Research and Instruction the following papers were read:

The Control of the Hipp Chronoscope in Psychometry, by J. B. KRAUSE.

Experiments in the standardization of control-instruments showed that a fall-screen can be standardized so that it will mark off a time interval correct to $.22\sigma$, with a mean variation of less than $.3\sigma$. Tests with a pendulum showed that the constant and the variable error are approximately one-half as large as those of the fall-screen. Data were presented showing the degree of uniformity of reading which can be attained when the instrument is properly adjusted. In a series of 4,000 records taken in groups of 160,—comparison made between chronoscope and fall-screen at the end of each group,—the average difference between readings at the beginning and the end of groups was found to be $\pm 1.008\sigma$. In a smaller group of similar records with pendulum control the average difference was but $\pm .36\sigma$.

A Finger Plethysmograph, by HENRY H. GODDARD.

This new form of the instrument was designed to avoid two defects in plethysmographs now in use,—loss of time in adjusting to the body, and impossibility of reproducing exactly a former adjustment. The problem is solved by attaching the instrument to the thumb-nail by a clip, and then adjusting the pneumatic system to the ball of the thumb by means of an easily legible micrometer screw.

Note on a New Method of Preparing Color Stimuli of a Definite Saturation, and its Application to the Study of Weber's Law, by J. H. LEUBA.

Dr. Horn and Miss Blake, of Bryn Mawr College, have published the results of a series of colorimetric determinations wherein was employed a new and ingenious method of preparing chromatic stimuli. The stimuli were composed of light transmitted through filters filled with colored solutions of different composition, and at different degrees of concentration. The method is commendable in that it permits of an accurate reproduction and a minute gradation of stimuli. Professor Leuba demonstrated the method in detail, and pointed out that it furnishes the means for a new confirmation of Weber's Law. The application of the method gives results which show that, within limits, the amount of color solution which must be added to produce a just observable difference in color, is a constant fraction of the amount of color-substance already present.

Student Apparatus for Courses in Experimental Psychology, by E. B. TWITMYER.

A demonstration of the methods employed at the University of Pennsylvania. Each laboratory group is supplied with a set of apparatus which becomes the personal property of the students. Many of the instruments are designed by Dr. Twitmyer, and constructed by the laboratory mechanician; they are simple in design and remarkably efficient in operation. The plan and scope of the course was described in detail.

A Demonstration of a Cheap but Efficient Air-Plethysmograph Constructed from a Lamp Chimney, by E. C. SANFORD.

The glass fits over the hand; it is closed at one end (the wrist) by a thick bandage of putty, and at the other end by a cork fitted with glass tubes for pneumatic transmission, and for the equalization of air-pressure.

In the session given to Research Problems the papers read were as follows:

Memory for Absolute Pitch, by J. W. BAIRD.

Tests made upon a remarkable case of pitch memory were described. The eighty-eight notes of the piano, presented in difficult order, were identified with an average error of about four per cent. The distribution of errors showed the middle and upper regions of the key-board to be most readily identified,—a result which was confirmed by chronoscope records of reaction-times. The reproduction of tones by means of the voice, and the Stern *Tonvariator* showed a surprising degree of accuracy. Tests were made with tones of various clang-tints; pipe organ (flute, diapason, reed and other stops), *Tonvariator*, tuning-forks (mounted and unmounted), Galton whistle, bells, glasses, rods, etc. The discussion emphasized the importance of eliminating the participation of relative pitch memory (or 'sense of interval') in the investigation of absolute pitch memory. (This paper will appear in full in the *Monograph Supplements*.)

The Fluctuation of Minimal Visual Stimuli, by C. E. FERREE. (To be published in full in the *American Journal of Psychology*.)

Experimental Studies of Reasoning Processes, by E. C. SANFORD.

A demonstration of a convenient method for the experimental investigation of the reasoning processes. Puzzles of various sorts, and problems involving the simple arithmetical operations furnish especially appropriate material. An exercise in long division, for example, is worked out in full by the experimenter, who then erases certain figures from divisor, dividend, quotient, intermediate products and remainders, and substitutes crosses for the missing figures. The subject is asked to determine what figure is represented by each cross. Such exercises facilitate at once the inspection of the subject's reactions, and his introspection of his mental processes. The results show that habit plays a prominent rôle in the method of attacking a problem, and also marked individual variations of method (confirming the earlier work of Lindley). All reasoning processes show a tendency to conform to a type of association under limiting conditions of apperception. The logical type of the syllogism is a formula which applies to many sorts of things—as to reflex actions (Pierce)—and not at all a true description of the psychical experience of inference.

Fluctuation of Attention to Cutaneous Stimuli, by L. R. GEISSLER. (Published in full in this number of the *American Journal of Psychology*.)

The Method of Just Perceptible Differences, by F. M. URBAN.

If a subject is required to make a series of comparisons between two stimuli, S_1 and S_2, his judgments vary without any apparent order or regularity, so that one is unable to predict what the judgment will be in a given experiment; but in a great number of experiments each judgment tends to occur in a certain percentage of all the cases. This indicates the formal character of random events, and we introduce the notion of a probability of a judgment of a certain type. This means that there exists a definite probability that the comparison of the stimuli, S_1 and S_2, will result in the judgment 'greater,' 'less' or 'equal.' These probabilities depend upon the amount of difference between the comparison-stimuli, and they may vary if the conditions under which the judgment is given are changed. An analysis of the experimental procedure which is called the method of just perceptible differences shows that for the just perceptible positive difference there exists the probability ½ that the judgment 'greater' will be given, and that there exists the probability ½ that on the just imperceptible positive difference the judgment 'greater' will not be given. Theoretically these differences are equal, and the cause of the differences which are always found in actual determinations is shown. Similar considerations hold for the just perceptible and the just imperceptible negative difference. The just perceptible negative difference is that amount of difference for which there exists the probability ½ that the judgment 'less' will be given, and the just imperceptible negative difference is that difference for which there exists the probability ½ that a 'less' judgment will not be given. The general notion of a just perceptible difference may, therefore, be defined as that amount of difference for which there exists the probability ½ that it will be recognized. Now the considerations which lead to these conclusions have certain practical consequences. The first and most important is, that one is not tied down to any specific order of presenting the pairs of comparison stimuli, if one records all the judgments. The second consequence is that one must not always use the same pairs of comparison-stimuli, but that one must 'vary the steps by which one approaches the threshold.' One finds, too, that the accuracy of the method of just perceptible differences compares favorably with that of the method of right and wrong cases, and that it does not require the hypothesis of any specific law of distribution. From this it follows that the method of just perceptible differences is not only highly serviceable for all purposes which do not require a high degree of accuracy, in which cases there is little objection to using it in the form which is now in use, but also that it is capable of a high degree of accuracy. The theoretical considerations are illustrated by the results of experiments with lifted weights. The results of the computation are compared with the actual observations, and it is found that they coincide in a remarkably high degree, even when the number of observations is rather small.

The Relation of the Four Taste Qualities to One Another, by J. W. BAIRD.

The experimental method consisted in establishing the liminal concentrations of sugar, hydrochloric acid, sulphate of quinine and sodium chloride, which could, under normal conditions, be identified as sweet, sour, bitter and salt. Then after exhaustion of the taste organs by strong solutions of each, the subject was asked to identify subliminal solutions and distilled water. A procedure without knowledge was employed, and 5 ccm. of each solution was given,—the subject being required to wash the liquid about in the mouth cavity during the process of identification. Several interesting results were obtained. Water after sweet tasted sour, after sour and after bitter sweet, and after sweet bitter. Subliminal bitter was identified after fatigue with sweet, with salt and occasionally with sour. Subliminal sweet and salt were identified after exhaustion with bitter. These results indicate that bitter is not unrelated with the other taste qualities as Kiesow affirms.

The Influence of Practice upon the Shortening of Reaction-Times (Sound Reactions), by CHARLES VUILLEUMIER.

Sixteen hundred reactions obtained from each of four subjects were treated by several statistical methods; the group averages and the mean variations all show the acquisition of celerity and uniformity of reaction with the progress of practice. The following table gives the averages of groups of 200 experiments for all four subjects, the results being averaged in groups of 200:

	I.	II.	III.	IV.
	117.06σ	154.97σ	163.15σ	134.11σ
	108.62	135.77	139.07	128.23
	102.6	111.83	129.95	123.75
	98.4	108.83	130.24	124.23
	96.4	99.77	128.51	111.44
	91.1	95.98	108.54	106.81
	92.2	96.73	113.7	103.78
	94.07	102.1	111.43	108.65
Grand average,	100.04	113.24	128.07	117.32

The mean variations varied as follows:

16.57σ	20.54σ	21.97σ	13.96σ
14.49	13.67	12.58	12.41
12.14	10.93	11.62	12.19
11.89	9.08	10.12	9.85

	9.40	7.77	9.09	9.59
	10.59	8.04	9.92	9.09
	10.33	7.47	9.77	9.95
	9.77	8.86	9.07	8.88
Grand average,	11.9	10.8	11.76	10.74

Practice shortens the reaction-time and it increases the number of those reactions which may be called normal.

At the meeting of the American Philosophical Society was presented the following paper:

The Effect of Imperceptible Shadows on the Judgment of Distance, by E. B. TITCHENER and W. H. PYLE.

This paper reported the results of a repetition and extension of the work published under the same title by Dunlap in 1900. The observer is asked to compare the lengths of two continuous sections of a horizontal black line upon a white ground. In certain series, unknown to the observer, the experimenter throws upon the background angular shadows, so disposed as to convert the two lengths of the line into the two parts of the Müller-Lyer illusion. The question at issue is whether the imperceptible shadows have any influence upon judgment.

It is shown, first, that these subliminal shadows, even raised almost to the limit of perceptibility, have no influence whatsoever upon the judgments of distance passed by five observers. It is shown further, that shadows, so weak as barely to hold their form distinct, exert an influence upon judgment, comparable with the influence exerted by strong shadows; there is no sliding scale of illusion-effect, varying with the clearness of contour of the illusion-motive. It is shown, thirdly, that the observer is able, by voluntary direction of attention, to resist the solicitation of a strong illusion-motive, clearly presented. So much the more then will he, under the conditions of Dunlap's experiments, resist the solicitation of an illusion-motive which he cannot see, of whose presence in the particular experiment he is ignorant, and which is left out of account in the instructions given him by the experimenter. It follows from the whole investigation that if the subconscious is to be received into experimental psychology at all, it must find some other means of access than these imperceptible shadows.

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University of Illinois.

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ON THE ANALYSIS OF THE MEMORY CONSCIOUSNESS FOR PICTURES OF FAMILIAR OBJECTS.[1]

By F. KUHLMANN.

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A. INTRODUCTORY.

1. *The Problem and Aim of the Study.* The present study is a continuation of a program begun in one previously re-

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[1] In this study acknowledgments are due to Prof. Joseph Jastrow, and to Prof. E. C. Sanford for their kindly interest and the generous opportunities offered to carry out the work; to Dr. W. F. Dearborn, and Mr. E. A. Jenner, University of Wisconsin, and to Dr. H. S. Britton, and Mr. George Ordahl, Fellows in Clark University, for their patient and expert work as subjects.

ported.[1] A few studies made by other investigators on the analysis of the memory consciousness were there briefly noted. In another place an attempt was made to outline what seemed to me the central problems in the general field of memory analysis.[2] For the present purpose it will be sufficient to recall a few of the more general questions in memory analysis with which the present study is chiefly concerned. Relatively speaking, nearly all previous memory studies have dealt with the quantitative aspect of memory, and a much smaller number with the determination of the types of mental imagery. The analysis of the memory consciousness is not at all concerned directly with the former of these problems, and the latter is only a very small part of its general aim. This general aim is threefold. First, the analysis of the memory consciousness into its elements. What different kinds of mental imagery, what organic sensations, and affective states occur in the mind from the beginning to the end of the process of the recall of a given thing? Secondly, the determination of the function in the memory consciousness of each of these elements. What is the order in which they appear, of what use is each in attaining the end that is desired, to wit, the reinstatement of the imagery that is wanted and the recognition of this imagery as correct or not? Thirdly, since the end product of a recall process is often a memory illusion, a prominent question in memory analysis is that of the nature and causes of these memory illusions. In the actual investigation of memory these are not independent general problems; for any study that is aimed mainly at any one of them will yield, and needs, considerable data with regard to the others, the second necessarily presupposing results on the first.

The results of studies of mental imagery hardly answer any part of these general questions directly. In our usual thinking we are not given free choice as to what sort of imagery we shall use in recalling things, because the occasion so often demands that we recall how a thing looks, or sounds, or tastes, etc. Thus, for example, to know that when one is given that choice one's imagery will be visual in 50% of the cases, auditory in 30%, motor and tactual in 15%, will not tell us much about the nature of the memory content in the total process of the recall of any one given aspect of a thing. Furthermore, the mental imagery studies do not tell us anything about the *function* of the different elements (different kinds of imagery), that enter such a process of recall. If I am trying to recall the nature of sounds I have heard I must recall auditory imagery, but it is not likely that my recall process will be made up entirely of a succession of auditory images. It will include most likely visual imagery of the things that produced the sounds, verbal imagery of their names or verbal description of some of their characteristics. It might include tactual, gustatory, and other imagery in addition. This secondary, non-auditory imagery which does enter may serve some purpose in the total memory consciousness, or it may not. In this given case of the memory consciousness for sounds, therefore, our double question of analysis into elements and determination of function would consist of determining what secondary imagery does enter, and what part it plays in the reinstatement of the auditory imagery and in the recognition of its correctness.

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[1] On the Analysis of the Memory Consciousness: A Study in the Mental Imagery and Memory of Meaningless Visual Forms. Psy. Rev., 1906.

[2] Problems in the Analysis of the Memory Consciousness. Jour. Philos., Psy., and Sci. Meth., 1906.

There are certain conditions which we know beforehand will determine in part both the kind of secondary imagery that will enter a recall process and its function. The first of these is the sense department in which the perceptive experience is given. A second is the degree of complexity and the familiarity of the material to be recalled. A third is the time interval since the original perceptive experience and the frequency of intermediate recalls. A good many others might be added, but these are the main ones. In the previous study referred to above meaningless visual forms were used, a group of which the subject committed to memory and recalled several times with detailed introspection and with increasing time intervals between the successive recalls. In the present study pictures of familiar objects were chosen because they present, as regards associated meaning and familiarity, a wide contrast to meaningless forms. The two classes of material together cover fairly well the range of differences in this respect.

In the present study, as in the previous one, the question of the function of the secondary and other associated imagery that entered the recall process was kept in the foreground, but the pictures of familiar objects offered the possibility of more and greater variety of imagery than was the case with the meaningless forms. The objects represented in the pictures, as may be seen in the cuts given below, were such as might appeal to all the special senses. Thus in the recall of some there was given the possibility, at least, of verbal, auditory, tactual, motor, gustatory, and olfactory imagery of the things represented; and this might appear in the recall process either before or after the visual image had been reinstated. But it may be stated at once that as a matter of fact auditory imagery entered only a few times, and then only in the case of one subject, and tactual, gustatory, and olfactory imagery never entered at all. This, then, is so far a characteristic of the recall of the visual aspect of things. In the analysis of the recall process as such we shall be concerned alone with the detailed description of the nature and function of other associated visual, verbal, and motor imagery, and shall consider how these were influenced by the lapse of time and the repetition of recall. To this will be devoted section C of the outline: "The Subjects' Methods of Recalling a Picture as a Whole." After the recall of the visual image of the picture as a whole has been considered, we shall proceed to the description of the recall of its further details. Here I shall take up especially what I have called the "criteria of correctness," which, as will be seen, was an essential factor in the process of recall, as well as in the recognitive consciousness, using this last term in a wide sense. The complete visual imagery of a picture as finally recalled was often wrong. These inaccuracies will be taken up in the section on "Changes in the content of the imagery," and their causes discussed.

2. *Method of Study and Procedure.* Two groups of pictures were used, and five subjects participated in the experiment.[1] The cuts show, on a slightly reduced scale, the character and the arrangement of the pictures in the groups. They were in black and white and the square spaces for the individual pictures were square inches.

In making the experiment a group was placed before one of the subjects at reading distance and he was given from six to twelve minutes in which to commit it to memory. He was told beforehand what the later procedure in the experiment would be, and that he should attend to the details in the pictures as much as possible in the time allowed. During the six to twelve minutes he was told when half his time had passed, and again when only one minute was left. The subject did not, after this, see the pictures again at any time. Immediately after memorizing a group the first introspective recall was required. The method of study and analysis was entirely that of introspective observation. In this immediate introspective recall the subject was first asked to describe in detail how he went about memorizing the group, and then to recall the individual pictures, one at a time, giving the order and nature of the imagery for each, the presence, character and use of associations, etc. This process, except the questions on how the pictures were memorized, was repeated three times after this with increasing time intervals, several days intervening between the immediate and the second recall, about ten days between the second and third, and about four weeks between the third and fourth.[1] In all but the immediate recall the procedure was divided into the two following parts: The subject was first asked to recall the group as a whole, naming the pictures, but not stopping to recall any details, letting the

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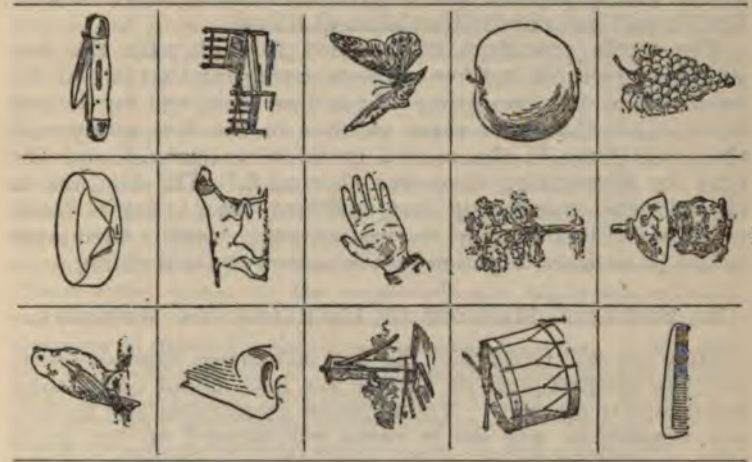
[1] As in the former study, the writer was the subject in a few weeks of preliminaries to determine minor matters of procedure, and was also one of the subjects in the regular experiment later.



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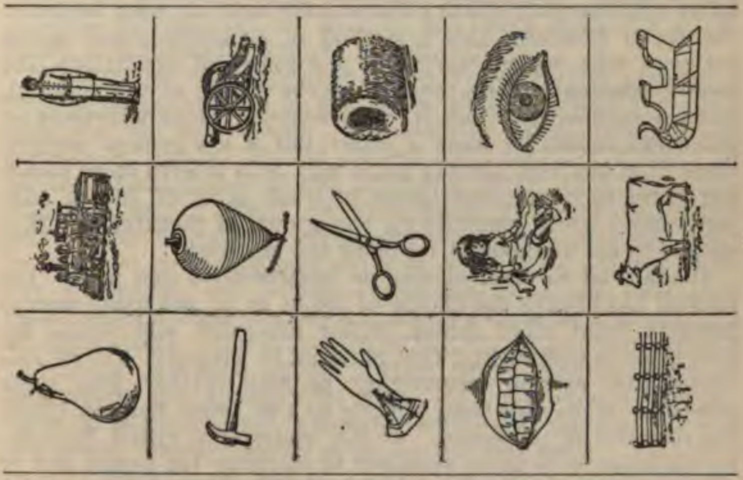

I.

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[1] These intervals varied somewhat, as a rigid adherence to uniformity would have involved great inconvenience to the subjects. But nothing in the experiment required greater uniformity than was secured.

recall take its own, passive course. When introspection on this was completed, which required giving the order and nature of the imagery, and the use made of it, for each picture, he was asked, secondly, to recall each picture in as much detail and as well as possible and to give his observations as before. In this part one picture was taken at a time.

The whole procedure was carried through with the two groups of pictures and five subjects, excepting that two of the subjects had only one group of pictures each, and for the last two subjects the upper three pictures in the first group, and the lower three in the second group were removed, and the time for memorizing them was shortened.[1] The aim was to allow for the memorizing just sufficient time to insure ready recall of all the picture at the first sitting. Usually then some would be entirely forgotten by the second or third sitting.

B. SUBJECTS' METHODS OF LEARNING THE MATERIAL.

How one will recall what he has memorized depends necessarily in the first place on how he has gone about committing the things to memory. The kinds of associations and secondary imagery he will use in recall will depend on the kinds made use of in learning the material. An analysis of the latter will therefore always throw some light on what occurs in the recall process. One's method of memorizing any given material will depend again on a number of conditions, the more prominent among them being the nature of the material, and the manner in which it is presented. Generalizations on this point cannot be made *a priori*, but a few general suggestions from analyses already made will help to give more meaning to the following description of what occurred in the subject's mind while memorizing the group of pictures, in other words, to his methods of learning the material.

If material for memorizing is presented to one sense, the subject has always the choice of using or of not using mental imagery from other senses, motor processes, and verbal descriptions in committing that material to memory. If we call these latter *aids* in memorizing, he may, further, give more attention to the aids than directly to the sense field in which the material is presented. If the material is visual, he may, for example, give more attention to learning the names and to verbal description, or some other aid, than to the visual aspect of the things presented and to visual imagery. From studies previously made it can be said safely that the subject will use quite different aids, and use them in different ways, according as the material is visual, auditory, olfactory, etc. Similar statements can be made in regard to the dependence of the methods of learning on the complexity and familiarity of the material, in whatever sense department it is presented. Also, the way of presenting the material will determine in part the procedure of the subject. If the different things in a group or list are presented simultaneously, he will do differently from what he will do when they are presented successively. Simultaneous presentation favors the use of associations among the objects presented, and will also make the subject voluntarily divide his time between perception of material and trial recall. A good many other matters of this sort might be mentioned. But this is not necessary, if it is clear that an exact analysis of all that the subject does while memorizing the material, when all the conditions are taken into account, should be made and kept in mind in order to help the understanding of many things about the recall process. Some matters about the methods of learning are quite naturally so general as to be independent of the particular subject and of many of the alterable conditions of the experiment. Others are entirely dependent on some minor point in the procedure. The following analysis of the subjects' methods of learning the material includes all the details that were definitely established in the present series of experiments.

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[1] The number in a group was determined simply by the time required for the introspective recall. A group of fifteen made a sitting of an hour and a half, which was found too fatiguing.

Without an exception, the subjects learned the pictures in order from left to right in the horizontal rows, and the rows from the top downwards. The regular arrangement of the pictures eliminated almost entirely any effort to commit to memory the positions of the individual pictures in the group. This took care of itself. It contributed further to the ease of memorizing by readily permitting associative grouping, and by fixing at once a definite order of procedure. Also without any exception, the subjects alternated learning with trial recalls several times during the six to twelve minutes allowed for memorizing. To determine in this way what could already be recalled and what needed further special attention usually took nearly half of the total time. While looking at the pictures the attention was always predominantly directed towards the visual, visually noting what the pictures were. Simultaneously naming them, and to a much less extent, describing details, might then incipiently accompany the visual process, or the subject might voluntarily attempt to keep in mind the names as he proceeded. In either case, the attention went first to the names in the trial recalls. With only one exception (one subject for one group of pictures) the effort in the trial recalls was to get first of all a series of names. Whether or not he would visualize the pictures at all when going over this series depended on the subject. The reason for this attempt to get a series of names was variously given. It was for the sake of preventing forgetting any of them altogether, or because it was easier to recall the names than to visualize the pictures successively, or for the sake of fixing the order of the pictures. The first seems to have been the main one. The ability to recall all of them in some form was first sought for. Only after this could be done would attention be paid to the details of the individual pictures.

Noting visually what the pictures are and their details is properly regarded as the direct method of making certain the recall of their visual images. The aids to this direct process were then, first, the names that were always associated with the pictures. Verbal description of the details, which was a quite common aid with the meaningless forms, occurred so rarely with the familiar pictures that it may be left out of consideration. Secondly, associations between picture and picture (depending more on the subject than on the nature of the pictures) were frequently aids in memorizing the latter so that none would be forgotten as wholes. Associations were much less sought for than was the case with the meaningless forms; were much more easily obtained, coming in usually of themselves and in unexpected ways; and were of a larger variety, most of them being of a different nature from those used in the memory of meaningless forms. Taking a distinctly psychological, rather than a logical point of view, they fall into a number of classes: (1) A mere felt connection between two or more pictures. The subject would not be able to report in what way they were connected, or anything further about the nature of the association beyond the statement of the presence of this emotional reaction aroused by regarding the pictures in question. (2) Putting several together into one class, describing them by one adjective or phrase. Accompanying this classifying was an emotional reaction of the same general nature as above. In just a few cases there was introduced in addition an extra visual image that in some way represented the class. For example, *glove*, *scissors*, and *muff* were associated with 'millinery,' and a millinery store was visualized. *Teeth*, *child*, and *eye* were associated with 'things pertaining to human beings,' and a person visualized. (3) Putting several together as parts of one complex scene. In this a visual image of a complex scene was used in which the things represented by the several pictures associated were present as parts. Thus, *fence*, and *cow* were associated by a visual image of a cow in a pen, of a cow fenced in. *Pump*, *hand*, and *butterfly* were associated by the visual image of the subject pumping at a hand-pump with a butterfly sitting on the ground nearby. In this class a verbal descriptive phrase might, or might not, accompany the visual scene. (4) Combining the names of the pictures into a phrase or a sentence. *Nose*, *dog*, and *bed* were combined into "The dog knows his bed." *Teeth*, *child*, and *eye* were combined into "Idol's teeth," regarding the child as a doll for this purpose. (5) With any individual picture various associative suggestions might occur without in any way connecting that picture with another. Excluding the names of things in the pictures, such associations with individual pictures were much more loosely connected with the images of the pictures than were the others. They, as a rule, did not seem to be an integral part of the total complex involved in the recall of the visual image of a picture and were regarded by the subject as "mere suggestions."

C. SUBJECTS' METHODS OF RECALLING A PICTURE AS A WHOLE.

In this section will be considered the nature of the mental content in the process of recall, and its function in that process, when an attempt is made merely to recall the pictures as wholes, not stopping to recall any details of the visual imagery of the pictures. A consideration of the changes dependent on the lapse of time and the repetition of recall will also be included. We shall be concerned entirely with the aids to recall, for the kinds of aids that the subject uses and the way in which he uses them constitute his method of recall for the given conditions. In case of the immediate recall, the recall process was in a very general way a repetition of what occurred in the mind of the subject while looking at the group of pictures and during the trial recalls while memorizing them; the same kinds of imagery, the same aids, reappeared in the immediate recall in about the same way. But even in this immediate recall there were some significant changes both by way of omissions and of additions, while in the later recalls the whole process was often quite different in content, and in the function of the elements in that content, from the original learning process. The analysis of that part of the recall process that will concern us in this section will have to do with the following: (1) Some factors determining the order of recall; (2) the associations; (3) verbal imagery; (4) certain visual antecedents, that were not visual imagery of the picture itself. The term 'associations' is used here in a restricted sense. Only the mental content that was classed under "associations" above, and which was divided into five different kinds of associations, will be considered under this heading. The nature of the results makes this method more convenient than the strictly logical one which would place nearly all the means of recall under the heading of associations.

1. *Some Factors Determining the Order of Recall.* As already stated, at each sitting, excepting the first one for a group, the subject was asked first of all to recall all the pictures while remaining in a semi-passive mood, letting the recall take its own course. He was requested to use a special effort only in inhibiting the recall of details of any one picture, if such a tendency should appear. The object of this was to determine the general mechanism of recall, as it would appear in the differences in the spontaneity of the imagery for different pictures, in the different classes of imagery for any picture, and in the presence and use made of associations. With this procedure, the order in which the pictures were recalled becomes significant. It shows some of the factors that are at work, and something about their relative efficiency in producing again the visual images of the pictures.

The general fact in regard to the order of recall was that at the first sitting the pictures were always recalled in the definite order in which they had been learned, from left to right and from the top downwards. At the second recall this order was already considerably broken up. At the third and fourth recalls it was changed still more, so that in individual cases little or nothing of the original order could be detected. Arranged in a table of distribution, all the cases taken together show only a general tendency to recall the pictures in the order in which they were learned, the rule being that any individual picture was more often than not recalled out of its exact order, but in the majority of cases not much out of its order. A minor tendency to recall them in the reverse order is then also detectable.

The order of learning was evidently, in the first place, determined in part by the reading habit. Also, in the recall there was sometimes present a general visual image of the rectangular group of squares in which the pictures were placed. With the presence of such a visual image the reading habit would incline the recall to take the order of the learning. This, because of the thoroughness with which the reading habit is fixed, must have been a more or less constant factor. The order of recall was then, in the second place, fixed, of course, through the working of the principle of contiguity. This was possible in two more or less independent ways. First, in giving a definite sequence of visual images, and secondly, in giving the same sequence in the series of names. Against these factors the influence of others finally broke up the original order of recall. The nature of the results as a whole makes it rather evident that this was due more to a recession of the former than to an increase in the inherent influence of the antagonizing factors. Of these latter several can be made out with certainty. (1) There was a difference in the inherent spontaneity of the imagery for the different individual pictures. Aside from associative connections, of special interest, of position, or of any other factor that could be detected by introspection, the images of some pictures came up more readily than others and tended to come up before others. (2) An association, apparently from a variety of different causes, might have a special prominence, be recalled first, and with it the pictures it involved. In a good share of instances the subject consciously sought for the associations when the visual image of none of the pictures rose at once directly. Again, in a number of instances the association was related to some personal experience of the subject, giving it a special spontaneity. (3) A general visual image of the positions of the different pictures, which was much aided by the regular arrangement of the squares, in the majority of cases preceded the recall of individual pictures. When for any reason a picture was recalled out of its order, there was a very strong tendency to recall next the one next to it. In this tendency, however, when the original order had once been broken, the one that followed the picture in question in the original order was shown no noticeable preference over others next to it. There seemed to be a tendency to get back to the proper starting place, an expression, undoubtedly, of the tendency to follow the reading habit which was made possible by the presence of this general visual image of positions, but which, in this case, broke the original order still more instead of helping to preserve it. The objective results and the subjects' introspections agree in making this general visual image of the arrangement and positions of the pictures a considerable factor in determining the order of recall. By inference, it would seem that this factor probably also accounts for the slight tendency to recall the group as a whole in the reverse order.

2. *The Associations.* (a) Their Uses. Twenty-four cases of associative connections that were made among the pictures while committing them to memory were of such a nature that the history of each could be traced through the four successive recalls. In the present description the figures that are given are based on these cases only.

The function of the associations needs to be considered only in the actual recall of the pictures, and it is assumed that when the subject was conscious of the association *before* the visual image or name of the picture appeared, the association then assisted in the recall. It might, of course, appear after the recall, and then add to the recognitive certainty as to the correctness of the recall; but it added to recognitive certainty so rarely, occurring only a few times, that it may be left out of account here. The subject did not seem to need this aid to the recognitive process; he was certain that the picture recalled was one of the group without it. In this respect the function of the associations was here essentially different from that in the memory of meaningless forms. The subject's general attitude in regard to their use in the recall was also different. As was the case in learning the group, so in the recall later the associations were much less sought for, were proportionally more numerous, and came in more of their own accord than they ever did with the meaningless forms. This produced in the subjects the general opinion that in many cases the associations were really not needed for recall, that the pictures would have been recalled without them.

The total number of associations that were used in the four successive recalls and whose course could be traced throughout, and the total number of pictures involved in these associations are given in the following table:

1ST RECALL.		2ND RECALL.		3RD RECALL.		4TH RECALL.	
Assoc.	Pictures Involved.	Assoc.	Pictures Involved.	Assoc.	Pictures Involved.	Assoc.	Pictures Involved.
24	63	15	33	18	32	16	41

From the previous description of the different kinds of associations, it will be remembered that an association usually involved more than one picture. The conditions of the experiment gave a total possible number of pictures that might have been involved in associative connections at each recall of 96. This gives some measure of the prominence of associative recall. The irregularity of the figures for the second to the fourth recall is evidence of a complexity of the factors involved; they cannot be adequately determined from present results. But in general it is clear that a good share of the associations used in the first recall are not so used in the later ones, approximately a third, on the average, have dropped out in the latter. From other results we might expect the presence of several factors that would influence the use of associations. First, with the repetition of recall, if it is frequent, the direct visual images of the pictures gain relatively in spontaneity and associations drop out. Second, with the lapse of time, if the recall is relatively infrequent, the visual images lose in spontaneity and require former associations for their recall. Third, with the lapse of time the associations are themselves forgotten.[1]

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[1] These three tendencies were found in the memory for meaningless visual forms. See pp. 330-333 of study referred to above.

(b) Their Changes. The general character of the associations in the successive recalls indicates that the two or three pictures that are thus connected, together with the imagery of the associations, constitute a complex of some degree of unitariness. The pictures 'belong together,' seem somewhat separated from the rest in the sense of possessing characteristics that the others do not share, the transition from one element in the complex to the other is very easy, and most of all, there goes with it an affective tone that is characteristic of these other qualities. The complex, however, is a loose and very unstable one. In any given recall, the repetition of such a complex, just as it had appeared in the previous recall, was a very rare occurrence. These changes may be considered under two classes. First, those taking place in the arrangement, relative prominence, and number of elements in the imagery of the associations themselves. Second, disregarding this first class, changes in the way and point at which the associations came in. Considering the latter first, the results show a process of a gradual elimination of the associations. The figures given above show that a good many that were used in the recall on the first occasion were not so used in the later recalls. But there are intermediate stages before they disappear altogether. Allowing for many exceptions, the order is as follows: (1) The association comes in before any of the several associated pictures are recalled. (2) The association appears after the first or second picture is recalled. (3) The association appears after all are recalled. (4) It does not appear at all. Taking the twenty-four cases of associations used in the first recall, and in the first of these four ways, through the second to the fourth recalls gives 72 instances in which these associations might have been used in the last three recalls as they were in the first. But in the last three they appeared in the four ways indicated. Their distribution for these four ways is as follows:

(1)	(2)	(3)	(4)
43%	22%	14%	21%

The other class of changes, the changes in the imagery that constituted the associations themselves, were equally prominent, and the direction of the changes with the lapse of time and the repetition of the recall could also be definitely traced. The predominant tendency was that of a rapid simplification of the imagery, though some other changes, leaving the total imagery equally complex, were also present in a much less degree. Simplification affected all classes of associations, visual and verbal, excepting those cases in which the associative connection consisted merely of combining the names of several pictures into a phrase or sentence. Other cases of verbal imagery made up of a phrase or short sentence would be very readily reduced to a single word or two. Complex visual imagery would be readily reduced to a single factor of the original. A still further simplification was frequent in the different recalls. In this the only representative of the original association was the 'feeling' that there had been some connection before. The subject would, in these instances, approach the recall of the several pictures that had been associatively connected before with the distinct, very clear consciousness that such association had existed, but would recall the pictures without the appearance of that former visual or verbal imagery that constituted what he called the association. Such an independent feeling of the existence of an association preceding the recall was the form in which the original association persisted in 17% of the cases in which an association was used in the last three recalls.

3. *The Verbal Imagery.* The verbal imagery falls into two classes: (1) The description of the details of the pictures, and (2) Their names.

(1) Description. Verbal description entered at two points in the experiment: (a) In learning the group of pictures the subject had incipiently to some small degree a tendency to describe the visual details. Then (b) at each recall his introspective report necessarily involved detailed description of how he visualized the pictures. These descriptions might thus have been expected to play a part in the later recalls as a means to recall or to reinforcement of recognitive certainty. But as a matter of fact, no description used in learning was ever used or present in later recalls. Verbal descriptions used in previous introspective reports were sometimes recalled; the subject remembered what he had said before, but, considering the results as a whole, this was quite exceptional. The general procedure of the subject was decidedly independent of any memory of what he had said at a previous sitting. A further rather significant fact was that when he did clearly recall a previous description he often put very little reliance on its accuracy as regards the points in the picture described. Present visual imagery, though in itself uncertain in character, seemed more acceptable than the clear recall of description, if the two in any way contradicted each other. On the other hand, when the recall of a description agreed with a somewhat wavering visual image that was recalled first, the verbal memory, with some exceptions, quite readily induced the acceptance of the visual as correct. It reinforced recognitive certainty. The recall of previous description as the sole basis of accepting any point about a picture as correct occurred only a few times.

(2) The Names. The name of the thing in the picture also entered the process of recall in two ways. First, naming followed quite reflexly the appearance of the visual image, the subject being entirely unaware of any intermediate imagery, visual or auditory, of the name. Second, the auditory image of the name entered either before or after the visual image of the picture appeared. In about three-fourths of the cases the subject could tell clearly the order of succession. These give a means of determining the rôle of the name in the recall of the visual image.

It will be remembered that in the trial recalls, during the learning of a group of pictures, the subject's attention went first to the recall of the series of names. This was, without exception, never the case in the recall later. In these the subject's attention was always directed to getting the visual images, either directly or through the associations. At each sitting each picture was recalled twice; first in the general recall of the pictures as wholes, and shortly afterwards in the recall of the details of each individual picture. The following figures give the general percentages of the number of times (a) the visual image appeared first, (b) the verbal appeared first, (c) the visual and verbal appeared simultaneously, so far as the subjects could judge.

	a	b	c
General recall,	77	15	8
Detailed recall,	86	11	4
Average,	82	13	6

This shows at once the very great predominance of the visual imagery over the verbal, and the rather small rôle of the latter in the recall itself. As to its use when it appeared after the visual imagery, it may be stated at once that the name never seemed to affect recognitive certainty at all. Its presence was useless in these cases. The percentages show also that the visual imagery gains in spontaneity with repetition more readily than does the verbal. Since in each case the subject had to name the pictures in his report as well as visualize them, the procedure should not have greatly favored one class of imagery over the other. But it is seen that in the detailed recall the visual is first in 9% more of the cases than in the general. This relation can be followed further by comparing the different recalls with each other, from first to fourth. The following are the percentages:

	1st recall.	2nd recall.	3rd recall.	4th recall.
Visual first,	76	76	89	89
Verbal first,	17	15	11	7
Simultaneous,	7	9	0	4

If, in connection with the relation of visual priority in the general and detailed recalls, it is remembered that in the first recall the general was omitted, it is seen in these last figures that the priority of the visual imagery increases considerably with repetition of the recalls. In the first recall the average for the priority of the visual imagery would be several per cent. less if a general recall had been included. Put into general form, the last two tables mean that with the repetition of the recall the verbal imagery is eliminated from a place where it is not absolutely needed. The visual imagery gains in spontaneity, as compared with the verbal, and is recalled directly. The verbal comes in only after the visual, at a point where it is needed for the purpose of description.

4. *Some Visual Antecedents.* Two kinds of visual imagery that often preceded the recall of the visual image of the picture itself may be given a passing notice. To the subjects their significance never seemed great. But in analyzing the results it appears that the one class at least may have been of considerable use. This is the visual imagery of the rectangular paper with the regular arrangement of squares in which the pictures were placed. The other is the visual image of a dark, quite characterless blotch in the position of the picture, and in some way representing it. The former was of somewhat varying character. Its presence or absence in the first place depended on the ease of recall, the lapse of time, and the absence of associative recall. In the later recalls it was much less present than in the first. Recall through associative connections tended to eliminate it, especially if the recall was easy. In general, it appeared most when there was some interval between the initial effort to recall and the actual recall. It varied in completeness and in the definiteness with which all the squares were simultaneously visualized in a single image. As the extremes, there was, on the one hand, a definite and clear image of the rectangular piece of paper in proper size, color and distance, with a less definite and complete visual image of its 12-15 squares. At the other extreme, this visual antecedent consisted of the visual image of a few squares only, with an indefinite margin, the subject not being conscious of the rectangular form of the piece of paper, of its color, or that it was paper, and it was less definitely fixed as to distance, usually farther off than the original reading distance, and considerably enlarged. It seems safe to attribute to this visual antecedent at least one important function in recall. It gave a means of guiding and fixing the attention. With this visual map before him, the subject could halt his attention and concentrate his efforts on the recall of one individual picture, and could rally his associative aids around this one position until

the picture was recalled. Without such a visual map of positions, or its equivalent in associative connections, he had no starting point, no clue, could not concentrate his efforts, and could do little more than simply wait and let the recall take its own, spontaneous course.

The visual image of a dark blotch in the place of a picture that preceded the recall of the picture itself was often present when the recall did not take place readily. No instance, however, occurred in which it involved any characteristic other than size, position and an indefinite roundness of form. Nothing in it distinguished one picture from another; it contained no clue to recall. Apparently it served no purpose like those of the other aids to recall.

D. THE VISUAL IMAGERY IN THE RECALL OF THE DETAILS OF A PICTURE.

We have now to consider the analysis of the recall process after some sort of visual image of the picture as a whole has been obtained, a visual image that has been sufficient perhaps merely to allow the naming of the object and the recognition of the picture recalled as one that belonged to the group memorized. The recall of the details was then a second step, although in individual cases a line could not always be drawn definitely between the two parts of the recall. The details of the simplest of the pictures used were too many to be included in one span of consciousness. The process of their recall was therefore necessarily a succession of imagery of some duration. This was decidedly a visual process. Practically no secondary imagery other than visual ever entered as an aid to getting the visual details after the visual image of the picture as a whole was once reinstated. We have, therefore, in the further analysis of the recall process to consider visual imagery alone. A prominent feature in this was the appearance of much imagery that was wrong. This wrong imagery we shall discuss in the section on "Changes in the Content of the Imagery," after taking up first the general character of the process of the recall of details independently of this wrong imagery.

1. *The General Character of the Process of Recall of Details.* (a) Differences in the First and Last Parts Recalled. A few items may be noted first of all in regard to what appeared first in the total imagery and its relation to what followed. The order of the recall of details brought out very clearly the degrees of difference in spontaneity and ease of recall for these details. The easiest was recalled first, and that ease was due not to previous associated imagery, but to the inherent character of the imagery itself. This was shown first of all by the fact that the details that were recalled first were in so many

cases always the same parts of the picture. Other characteristics point to the same general fact. In a good share of instances the first details recalled and what followed were quite distinct in either one of two other ways. First, there might be a considerable interval following the first part before the rest could be recalled at all. Second, the imagery for the first part would be fixed in its character, the following part wavering in both its content and in the degree of certainty that went with it.

(b) The Criteria of Correctness. The recall of details was essentially a process of reconstruction. On the whole the final product that was accepted as correctly representing the original picture was the result of many eliminations and variations of visual details that suggested themselves spontaneously. Usually it was not so much a matter of filling in details somehow or other as it was a matter of deciding which of the details that were readily imaged were the correct ones. The general nature of the process is, therefore, best described further by considering the criteria of correctness and the manner of their use.

Although customary usage applies the term 'recognition' to all such criteria, the present results call for some discrimination. The form and content of consciousness that led the subject to accept a certain detail in the imagery as correct were of several different kinds. (1) Direct recognition may be mentioned as one of these. The term is used now in its most technical sense. In these cases the subject regarded the detail in question as right or wrong directly; he was unable to detect anything, in the character of the imagery itself or in the total process, on which acceptance or rejection might be based. Such recognitive consciousness was not, however, necessarily an immediate result of the imagery. It might appear only after some considerable attention to the imagery, and attempt at decision in some other way. Direct recognition was the most important way of deciding, but it was oftener than not accompanied by other factors as aids. (2) Inherent spontaneity in the imagery frequently led to its acceptance, especially in the absence of other criteria. The influence of this was brought to light clearly in cases in which a detail was visualized in two different ways, neither of which was recognized directly as right or wrong, and either of which might have been correct, so far as other details were concerned. If in such cases there was a distinct difference in the ease, vividness and spontaneity with which the two images appeared, the one having these qualities in the most marked degree was usually accepted, or at least favored as regards correctness. On the other hand, spontaneity and vividness were very readily rejected as criteria of correctness in favor of even a slight degree of recognitive consciousness attaching to competing imagery. In numerous instances the subject described certain imagery as very easy to get and vivid, and yet rejected it in favor of other imagery which he faintly recognized as correct, and which at the same time might have been very difficult to get and to hold. (3) The absence of rival imagery increased the tendency to accept whatever did present itself. The effect of rival imagery was shown in instances in which the subject at first recalled a detail in a certain way and accepted it as correct, but a moment later became doubtful of it or even rejected it when another suggested itself, which in itself seemed to have nothing more in its favor than that it was a possibility. In general, the suggestion of more than one possible way of recalling a picture created doubt where otherwise there might have been considerable certainty. Another tendency shown was that of rival imagery to drop out with the lapse of time and the repetition of recall. Where such was the case it was the rule of the subject to accept as correct what he did recall, though he had previously doubted its correctness in the presence of rival imagery. (4) Something in the nature of a process of inference was a very prominent method of deciding whether the imagery of a certain detail was correct or not, and was also a factor in producing that imagery in the first place.[1] This was so closely interwoven with the other factors that it was throughout difficult for the subject to detect its presence, or to say whether he believed a thing was so because of a direct recognition of its correctness, etc., or because he inferred it to be so on account of other things that he did so recognize or know. A detail might, of course, be inferred to be right for many different reasons. The results hardly allow any classification of the grounds for such inferences. Two sources, however, can be made out. First, the subject would recall certain details and be certain of their correctness. With this accepted, the nature of other details often followed necessarily. Secondly, in some cases he inferred details in the picture from what he knew of the thing represented by it, without any connection with details actually recalled. It is very evident, when other results in this connection are taken into account, that in very many instances some such process as this guided the recall and rejection or acceptance of imagery when the subject was not in the slightest degree aware that this was his method. Clear cases in which the subject reported that he was inferring rather than remembering were very few, less than fifty in all the results. And in many of these he discovered this fact only after special questioning. On the other hand, memory illusions were numerous, but the details in these illusions were almost without exception wholly consistent with themselves. Though the subject constructed and accepted as correct a total image that was quite different from the picture, the construction was consistent with itself. It is clear, I think, that if that construction had been guided alone by direct, but in this case false, recognition and the other criteria of correctness already mentioned, it would frequently have resulted in a group of details for a picture that would have been quite inconsistent with each other.

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[1] The term inference, for want of another, is used here in a wide and loose sense, and is not to be defined in the formal way of the logician.

The criteria of correctness changed somewhat with the lapse of time. In the first place, the general spontaneity of the imagery decreased. After the second or third recall it was evident to all the subjects that the pictures were recalled with much less ease, clearness, and detail. Second, the intensity of direct recognitive consciousness decreased. This took place, however, apparently from two quite different causes. For a few of the details that had always been very readily recalled and with perfect certainty as to correctness, the imagery came to be accepted later in a rather matter of course way, an attitude which, in the perception of familiar objects, is described as 'cognitive' rather than 'recognitive' consciousness. On the other hand, more frequently the intensity of recognitive consciousness decreased because of a loss of memory, the subject being less rather than more certain of the correctness of the details in question, so far as direct recognition was his means of judging. Thirdly, in some cases rival imagery dropped out. Details that were at first imaged in more than one way, the subject being uncertain as to which way was correct, were later imaged in one of the former ways only, the old alternative not occurring to the subject at all. In other cases rival imagery appeared in the later recalls that was not present at first. This seemed in general related to a decrease in certainty, from whatever cause, for some details. Certainty at first for a given detail barred out rival imagery. Later uncertainty gave the possibility of that detail being imaged in other ways and with an equal claim to correctness.

2. *Changes in the Content of the Imagery.* We may consider next the general character of the changes that appeared in the content of the imagery with the lapse of time. By changes in the content is meant now wrong imagery in the later recalls for details that were in the first recalls imaged correctly. These changes fall conveniently into two classes. (a) Imagery that presented itself more or less spontaneously, but which the sub

ject regarded as wrong. I shall call this conscious change. (b) Imagery that was wrong, but which the subject regarded as correct, illusions of memory. For the purpose of description these may be kept separate. But we shall see later that they are more closely related than their separate description might indicate.

(a) Conscious Changes. Following distinctions made in describing the criteria of correctness, the conscious changes in the imagery were of two kinds. First, the changes that were directly recognized as wrong. Second, the changes that were inferred to be wrong because of other known facts. These were the two methods that the subject was conscious of using in judging imagery. By far the greater part of the cases belonged to the former. As stated before, the recall was a reconstruction process consisting essentially of eliminating wrong imagery and deciding on what was wrong and what was correct. The tendency for imagery to come up that was at once recognized as wrong increased decidedly with the lapse of time. In some individual cases it bore the further characteristic of a high degree of vividness and persistence. Although at once regarded as wrong, the subject had difficulty in dispelling it. In the case of one subject such imagery was so prominent as to attract his attention and occasionally call forth generalizations on his own part. Thus in the third recall of one picture he said, "I find it very difficult to get a visual image that can be accepted. Various kinds of details come up that are at once recognized as wrong, but which, in the inability to get correct ones, are persistent." In the fourth recall of another picture he notes: "A number of visual details come up that are at once recognized as wrong. The visual imagery of these details comes up readily and clearly, while at the same time different details that are recognized as right are vague, hard to get and to hold." Such descriptions never occurred in the first recalls, and only a few times in the second, while they are frequent in the third and fourth. The generalization of this subject is verified by many concrete descriptions that state just what the imagery was and the whole process that ended in the acceptance of certain details as the correct ones. There was, however, apparently considerable individual difference among the five subjects on this point.[1]

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[1] This individual difference appears in the introspective notes, when they are taken literally. I am not certain, however, but that the subjects were more alike in this respect than their own notes would indicate. Occasional special questioning showed that their *habits of introspective observation* were different, and in such a way as to account for a difference in their reports where the process of recall might have been really quite the same. In all there was a strong

What was said above of the grounds for inferring imagery to be correct holds true in quite the same way of the grounds for inferring imagery to be wrong. Imagery was inferred to be wrong from various reasons. To the two sources of inferences in the former case must be added now a special class, inferences based on the memory of a generalization made about the pictures. There were no colors in any of the pictures, and, of course, no motions. The subject could not forget these general facts. In the later recalls the things in the pictures were very frequently visualized in colors, and some of them as moving. These characteristics of the imagery were, of course, readily detected to be wrong. Every one of the pictures was visualized in colors at one time or another in the later recalls by one subject or another. These colors were always the natural colors of the objects represented. A further peculiarity was then the fact that in some cases only a part of the object was visualized in its natural color, while the rest was imaged in the black and white of the picture. In the picture of a child, for example, one subject imaged its hair as brown, with no further color. Another subject imaged a ribbon in pink, with no further color in the dress. In a picture of a human eye the iris was imaged as blue, with the lids and brow in the black and white of the picture. The ends of the handle of a jackknife were imaged in brass color, the rest in black and white. The drumsticks with a drum were visualized in wood color and black paint, the drum itself in black and white, etc.

The objects were also imaged in motion, but more rarely than in colors, a fact evidently dependent on the nature of the object, for not all the objects pictured were mobile. There is some slight evidence also that those objects in which motion might have been imaged were not imaged in that way so readily as in colors. I quote a few of the more striking cases, which reflect the general character of this class of conscious changes. Third recall of a picture of a locomotive: "With

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tendency to attend only to the final product of the process of recall, the imagery accepted as correct, and a corresponding disregard of the process by which this had been reached. Quite frequently the steps in the process were forgotten by the time the end product had been described. This fact has an interest independent of its present connection; for the strong tendency to direct the attention in this way seems to be an illustration of the fact that in everyday life we rarely are interested in *how* we attain an end mentally, but only in what we accomplish. In memory, especially, we have no practical interest in how we remember, but only in what we remember. The point of present importance in this is the fact that the different subjects did not succeed equally in directing attention to the process, the antecedent imagery, as well as to the product.

further attention to the imagery the locomotive begins to move, especially the drive wheels and the piston rod. I see the puffs of smoke and steam. It is almost impossible to visualize it as standing still." Third recall of a picture of a top: "The visual image of a running top is persistent. With continued attention, it begins to wabble and sway, like a running top that is stopping." Fourth recall of a picture of a dog: "There is a little difficulty in visualizing the position correctly. The dog in the image keeps turning its head around to look at me." Fourth recall of a picture of a cow: "With further attention its head turns towards me. I see it move, with the added feeling that I am visualizing a real, live cow. This sort of imagery persists, and the visual image of the correct position comes out only in short flashes." The persistence of the motion and the relative inability to image the object as stationery was characteristic of the imagery of one subject only, and with him it was not true, of course, of all the pictures; but only one of the five subjects never reported motion in any of his imagery. Individual differences were greater in this than they were for visualizing the things in their natural colors.

It was stated that these conscious changes in the imagery increased with the lapse of time. Most belong to the third and fourth recalls. No imagery of motion belongs to the first recall. Taking the more prominent cases, 99 in all, of visualizing color or motion, gives the following distribution for the different recalls, first to fourth:

1st Recall.	2nd Recall.	3rd Recall.	4th Recall.
10%	19%	29%	42%

The conscious changes in the imagery directly recognized as wrong (the class mentioned earlier), because of their nature and number were too incompletely recorded in the notes to justify giving figures. But the record as it is, and the occasional generalization on the part of one of the subjects, indicate that their course was much the same as was that for color and motion.

Without discussing the conscious changes at this point, most of what is included in their description may be brought together under one generalization. The imagery tends, with the lapse of time, toward the imagery of the object represented by the picture, and with this change takes on characteristics that belong to the object, but which are not represented in the picture. It will be helpful to keep in mind this generalization in considering the next class of changes in the content of the imagery, the memory illusions.

(b) Memory Illusions. If the term were taken in its strictest sense it might be demanded that only those cases should be called memory illusions which were due to false recognitive consciousness. I shall, however, not so limit the term, but shall regard all wrong imagery that is accepted as correct as memory illusion, no matter what the criterion for acceptance has been.

The general frequency of illusions could not be determined definitely in the present study because of the fact that time never permitted the subject to give a really complete description of the details of his imagery. But taking the results as they stand, they show a large percentage of illusion. Its amount varied much with the different subjects, and more still with the different pictures. For some pictures some subjects never made any misstatements. For other pictures some subjects, in individual cases, described three-fourths of the details wrongly.[1] Taking the more marked instances of memory illusions, 280 cases in all, we find the following individual differences among the five subjects:

Subjects,	1	2	3	4	5
Per cent. of illusion,	17%	25%	9%	29%	32%

The conditions of the experiment were the same for the first three subjects; for the fourth and fifth the number of pictures in a group and the time allowed for learning a group were less, and the intervals between successive recalls were greater. The individual differences were probably due, in part, to this. Dividing the subjects into two groups accordingly, and figuring the percentages separately for each group gives:

Subjects,	1	2	3	4	5
Per cent. of illusion,	33%	49%	18%	42%	58%

The first subject described less of the details in the pictures than any of the others. The third gave his introspections in considerably more detail than any of the others, though not describing more of the details in the pictures. This led to greater discrimination in the characteristics of the imagery, etc., and made him more guarded in his statements as to what he really accepted as correct. Nothing further was found that might account for the individual differences in the amount of memory illusion.

There was some data also on the relation of the amount of memory illusion to the lapse of time and the repetition of recall. At least the first recall compared with any of the later ones gives considerably less illusion. Taking the same 280 cases used above gives the following distribution for the different recalls:

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[1] Several recent memory studies give results on the frequency of memory illusion. They show very great variations in the amount of illusion as dependent on the different kinds of material to be remembered, and other conditions. In the general average it amounts to about 20%. I have summarized most of these results in another place. See "Recent Studies of Normal Illusions of Memory," *Am. Jour. Psy.*, 1905, pp. 389 ff.

1st Recall.	2nd Recall.	3rd Recall.	4th Recall.
15%	30%	25%	30%

The results do not permit a definite statement as to why the amount of illusion did not increase regularly with the lapse of time, but several factors should be taken into account. First, a number of the illusions that were present in the first recall remained fixed throughout. Secondly, in the later recalls many of the details in the pictures were forgotten altogether, thus eliminating the possibility of making misstatements about them. Thirdly, the efficiency of the different criteria of correctness did not decline at the same rate as did the ability to recall details at all. Thus the amount of illusion in any given recall might be more or less than it had been previously according as these various factors were related to each other at the given time.[1]

An attempt to classify the memory illusions for the sake of describing their general character from the standpoint of the details in the pictures that were changed soon revealed the fact that they were literally of all sorts. The range of their variety seemed limited only by the possibilities of the things represented by the pictures. Any change in the object represented, which would not make it different from what the object ever actually is, might be introduced. Different groupings bring out different aspects of the tendency to illusion. Thus (a) with reference to the pictures described, details not in the picture might be added, or the presence of some that were there might be denied, or the number of parts of one class in a picture changed. The relative position of different parts were changed, the form of a part described differently from what it was, or the position of the picture as a whole changed so as to give a different view of the object represented. Taking another standpoint of classification, (b) with reference to their permanency, some remained unchanged, as already stated with regard to some of those that appeared in the first recall. In a good many cases the subject wavered back and forth between two statements, one or both of which might, in a previous recall, have been regarded as uncertain. A smaller number changed to a third and even fourth characteristic, without reverting to a former one. Classifying them (c) with reference to the way in which they entered, three sub-classes may be noted. In the majority of cases the details in question were simply visualized wrongly but 'recognized' as correct. No immediate clue to their origin was given. Many belonged to a class often noticed by casual observers. Details would at first be described wrongly but with uncertainty, or regarded as mere irrelevant imagery minus all memory sanction. Later the same description would be given, but without the previous uncertainty; it would be regarded as correct. A few clear cases occurred in which the subject simply reasoned wrongly in reconstructing his total image. He inferred that a detail must be so, on the basis of what he did remember of the picture or knew of the object, overlooking some other possibility.

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[1] Some figures from other memory studies show something of the relation of the amount of illusion to the lapse of time. These all agree in showing some increase with successive recalls. See summary of studies of memory illusion referred to above, *Am. Jour. Psy.*, 1905, p. 393.

Figures may be given for the classification from the first of these three standpoints (a) above. For they will reinforce a generalization of importance already suggested with reference to their causes. Taking the 280 cases used before, three classes may be made that cover all but 17% of the total number. These are (1) relation of parts changed, (2) the form of a part changed, (3) the position of the picture as a whole changed, giving a different view of the object represented, (4) the miscellaneous group containing the remainder of the cases, details added, details that were present denied, and the number of a class of things in a picture changed. The percentages of cases belonging to these classes are then as follows:

1	2	3	4
16%	34%	33%	17%

In considering these figures it must be noted that ten of the thirty pictures used were of such a nature as could not be affected by the first class of illusions, their being no parts to the objects represented whose relations could be changed and still leave the objects as they ever actually are. On the other hand, it should be remembered that there was much less occasion for the third class to be numerous than there was for the first or second; for there were necessarily much fewer whole pictures than there were parts in all the pictures. This brings out the fact that there was a specially strong tendency to illusion of the third sort; we may safely say, more than twice as great as for any other of these classes, although only 33% of the cases belong to it.

(c) Causes of Changes. A determination of the special causes of the changes in the content of the imagery with the lapse of time is in the present study for the most part a matter of interpretation. Bringing together a number of salient features that the general analysis has revealed will throw some light on this question. In the first place, it seems quite evident that the conscious changes and the memory illusions are both due to one and the same general cause. This is expressed in the generalization already made that the imagery of the picture tends to the imagery of the object represented by the picture. Whether or not the change will be a memory illusion or the subject be conscious that it is a change depends on special and largely quite incidental conditions. The validity of this generalization follows from the fact that both the conscious changes and the memory illusions were of almost unlimited variety, but were never inconsistent with the possibilities of the object. As stated of the illusions, any change might occur which was not different from what the object ever actually is. It is reinforced by the special prominence of the imagery of color and motion, and by the predominance of memory illusions that involved a changed position for the object, the object turned around somewhat as a whole. The last could occur for the imagery of the object without changing the object, but it could not occur for the imagery of the picture without changing the picture.

Some reasons may be given why there should be this tendency for the imagery to change to that of the object. The first three are facts of observation in the present study. In the first place the general spontaneity of the correct imagery, the imagery of the picture as seen, declined with the lapse of time. The more this was true the more was given the possibility for imagery of the object to present itself. Secondly, spontaneity was to some degree a criterion for accepting imagery as correct and lack of it for rejecting it as wrong, giving a further cause for the right imagery to drop out and the wrong to remain. Thirdly, the intensity of direct recognitive consciousness decreased, and thus permitted the elimination of the right imagery in favor of other imagery that was wrong. These are negative causes, favoring the decline of the imagery of the picture as seen. On the other hand, it may be said that the objects represented rather than the particular pictures of the objects were more matters of everyday experience of the subject. The two were undoubtedly somewhat different in every case. Thus the imagery of the object was the more usual, customary, habitual, and the mind in making the substitution simply followed the law of habit. This is a generalization that has already been made from other memory studies, and the tendency in question may be accepted as a factor here. But there are additional reasons why this should be so for the memory for pictures. Pictures as such have not the interest and emotional coloring that belong to the objects. Such emotional coloring, when it takes possession of consciousness, brings with it the visual imagery with which it is connected. Again, in so far as meaning and interpretation is read into the picture at all, so far, of course, the picture ceases to be what it is and becomes the object. The picture just to this extent creates the tendency to substitute the imagery of the object in its natural setting.

A few things further may be noted in summary in regard to what determines whether a change that has taken place in the imagery shall be a conscious change or memory illusion. We have found two general checks to illusion when wrong imagery appeared. First, recognitive consciousness, although declining in efficiency, is more persistent than the original spontaneity of the correct imagery. Wrong imagery may increase in amount and persistency, but the larger share of it is at once recognized as wrong. This is in accord with the fact found in other memory studies which shows that more things can be recognized as belonging to a previous experience when given a second time together with others than can be directly recalled without such second presentation. Secondly, inference from other remembered or known facts prevents illusion. Clearly this is what prevented imagery of color and of motion from becoming illusion. If some of the pictures had been colored, or if they had been objects, some of them in motion, mistakes would undoubtedly have occurred by attributing color or motion to individual cases where none had been present. As it was, the subject could hardly forget that no color or motion belonged to any of the pictures. In the case of imaging the object represented in the position represented in the picture there was no such check. The object could be imaged from any side or turned in any way. No generalization that might have been remembered applied, and we have consequently many illusions belonging to this class.

Finally, the varying relations of all these factors that produce changes in the content of the imagery account for the fact that the course of the increase in memory illusion through the four recalls was not the same as was the course of increase in the conscious changes.

E. SUMMARY.

In the experiments above described the subjects all learned the pictures in regular order from left to right and from the top downwards. They also all alternated looking at the pictures with trial recalls, giving about half the time to the latter. Their stated purpose in this was to determine what needed special attention and what could already be recalled. While looking at the pictures the attention went predominantly to noting the details visually. In the trial recalls, however, the effort was predominantly to recall the series of names of the objects represented. Calling the visual method the direct method of learning, certain aids to the visual process were used. These were the names of the things in the pictures, verbal description of their details, and associations with individual pictures and between different pictures. The use of verbal description for details was very rare. The use of associations was abundant. They were easily obtained, and were of a large variety.

Factors determining the order of recall of the pictures gave some evidence as to the factors present and their relative efficiency in the recall of the pictures as wholes. The order of learning was followed in the first recall, but this order broke up gradually with the lapse of time. Factors that tended to break up the original order were: (1) Differences in the inherent spontaneity of the imagery for the different pictures; (2) Special prominence of the imagery of associations; (3) A general visual image of the regular arrangement of squares in which the pictures were placed.

Associations were abundantly used in getting the visual image of a picture as a whole in the first recall. Approximately a third of them were *not* so used in the later recalls. When the associations appeared after the visual images of the pictures, they practically never reinforced recognitive certainty; they were not needed for this purpose. Changes occurred in the nature of the associations and the manner of their use with the lapse of time. The imagery of the associations together with that of the associated pictures constituted a loose complex. Time produced a gradual elimination of the associations from this complex, which, with many exceptions, ran roughly as follows: (1) The association appeared before any of the associated pictures were recalled. (2) It appeared after the first or second was recalled. (3) It appeared after all were recalled. (4) It did not appear at all. A second class of changes consisted in a rapid simplification of the imagery of the associations. In this a final stage before complete elimination was, in 17% of the cases, the mere 'feeling' that there had been some association before.

Verbal imagery used in recall was that of the description of details, and naming. Verbal description used in learning was never used later in recall. Verbal description used in an introspective recall was sometimes remembered in a following one, but the subject made little use of such verbal memory, and put little reliance on its accuracy. The naming of the thing in the picture followed its visual image reflexly, or an auditory image of the name preceded or followed the visual image. When it followed the visual it never influenced recognitive certainty. In 13% of the cases it preceded the visual image and was thus used in recall. In 82% the visual preceded, and in 6% the two seemed simultaneous. The priority of the visual image increased about 10% during the later recalls, as compared with the first.

Two kinds of visual imagery, aside from visual imagery involved in associations, often preceded the visual image of the picture. These were a visual image of the rectangular piece of paper with its regular arrangement of squares, and the image of a dark, otherwise quite characterless blotch in the place of a picture in a square. The former varied considerably in its general character. Its main function seems to have been that of guiding and fixing attention in the effort to recall individual pictures. The latter did not vary in character for different pictures; it served no function that could be determined.

The recall of details of a given picture was necessarily a process of some duration. Some differences in the character and manner of recall were found for what appeared first and last in that process. The part recalled first consisted of the easiest details, which were apt to be the same in successive recalls, with the nature of the content of their imagery fixed, and the subject certain as to its correctness. For the last part the details entering varied more, the nature of the imagery was not so fixed, and the subject was less certain. The two were often separated by a considerable time interval.

The recall of details was further a process of *reconstruction*, a reconstruction by means of eliminating wrong imagery more than one of producing imagery that was right in the first place. The criteria by means of which imagery was judged right or wrong or because of which it was accepted or rejected thus become the essential part in the actual recall process. Several such criteria were made out. (1) Direct recognition, using the term in its limited, technical sense. (2) Special inherent spontaneity of the imagery frequently led to its acceptance, especially in the absence of other criteria. But this characteristic was very readily discarded as evidence of correctness in the presence of even a very slight degree of recognitive consciousness attaching to other imagery. (3) The absence of rival imagery increased the tendency to accept whatever did present itself. (4) Inference, using the term in a very loose way, was a prominent method of deciding on the correctness of imagery. Without special and minute attention, the subject found it generally very difficult to discover that this had been his method in cases where actually it was so. Inferences were made on the basis of details already definitely recalled, and on the basis of what the subject knew about the things represented in the pictures. Indirect evidence from the memory illusions also indicates that this played a large rôle in recall. These several criteria changed somewhat with the lapse of time. First, the general spontaneity of the correct visual imagery decreased, Second, the intensity of recognitive consciousness decreased. Third, rival imagery dropped out in some cases; in other instances it appeared where it had not been before.

Changes in the content of the imagery that occurred may be described as: (1) Conscious changes, wrong imagery which the subject knew to be wrong, and (2) Memory illusion. In the majority of cases of conscious changes the subject knew the imagery to be wrong by direct recognition. Such imagery increased very much with the lapse of time, and had in some cases the further characteristic of a high degree of persistence. In a second class of conscious changes the imagery was inferred to be wrong from other data recalled or known. Among these the tendency to visualize color and motion in the pictures is of special significance. These were always the natural colors and motions of the objects represented. This tendency to color and motion also increased quite regularly from 10% of the total number in the first recall to 42% in the fourth. The memory illusions showed great variation in their general frequency for the different subjects and for the different pictures. Fifteen per cent. of the total number occurred in the first recall, with 30%, 25%, and 30% for the second, third and fourth recalls respectively. There was some additional evidence that a changing relationship of several factors, militating both for and against the illusions, determined their relative frequency at any given time. From the standpoint of the details in the pictures that were changed the illusions were limited in variety apparently only by the possibilities of the objects represented, any change being likely to occur that did not make the object different from what it ever actually is. Making an arbitrary classification from this standpoint showed that there was a specially strong tendency to illusion of the position of the object represented, *i. e.*, a changed view of the object.

One and the same general cause produced both conscious changes and memory illusions. This was *the tendency of the imagery of the picture to change to the imagery of the object represented.* Special reasons why this should occur are: (1) The spontaneity of correct imagery declined with the lapse of time. (2) Spontaneity, or lack of it, was to some degree a criterion for accepting imagery as correct or for rejecting it as wrong. (3) The intensity of direct recognitive consciousness decreased. (4) The object rather than its picture was more a matter of everyday experience, and its imagery readier, and more habitual. (5) More interest and emotional coloring belongs to objects than to their pictures. (6) To the extent that the picture represents and suggests the object the imagery of the object is already given.

THE PSYCHOLOGY OF HUMOR.

By L. W. KLINE.

OUTLINE.

I. Introduction.
II. Grouping of theories.
III. Humorous and non-humorous stimuli.
IV. The nature and origin of humor as a mental process.
V. Functions of humor.
    a. Psychological
    b. Physiological
    c. Biological
    d. Sociological.

I. INTRODUCTION.

Voltaire is said to have observed that Heaven has given us two things to counterbalance the many miseries of life,—hope and sleep. To these Immanuel Kant adds a third, laughter. No stimulus, perhaps, more mercifully and effectually breaks the surface tension of consciousness, thereby conditioning it for a new forward movement, than humor. It is the one universal remedy; a medicine for the poor, a tonic for the rich, a recreation for the fatigued, a beneficent check to the strenuous, a shield to the reformer and an entering wedge to the recluse. Barter and trade make a liberal use of it. A German writer observes that it is a parachute to the balloon of life. To change the figure, it is a switch on the railway of life preventing human collisions. It is a universal solvent to human temperaments, and like a touch of nature makes the whole world kin.

"Xenophon reckons that the man who makes an audience laugh has done a lesser service than the one who moves it to tears, but the comedian Philippos, when Socrates asked him of what he was proud, declared, 'I believe that I ought to be proud of my right to the gift of arousing laughter, as Kallipedes, the tragedian, of his art in causing tears.'"[1] Lycurgus even erected a statue to the god of laughter.[2] Cicero says that "it certainly becomes the orator to excite laughter; either because mirth itself attracts favor to him by whom it is raised, or because all admire wit—but chiefly because it mitigates and relaxes gravity and severity, and often by a joke or a laugh breaks the force of offensive remarks which cannot easily be overthrown by arguments." It has remained, however, for the man of modern science and letters to indicate more specifically the problems involved in the origin and nature of humor. I need hardly state that the theories have multiplied in number and refinement to the point where their enumeration becomes tedious. It is a part of the purpose of this paper to go over the field with the hope of discovering a point of view that will resolve the seemingly diverse theories to a common basis and to restate more explicitly the psychological nature of humor.

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[1] Nick, Fr.: Narrenfeste, Bd. I, 2. (1861.)
[2] Cicero: Oratory and Orators, p. 289.

II.

Grouping of Theories.

Groos[1] has attempted to reduce the multiplicity of humor theories to two, "that of the feeling of superiority and that of contradiction." Ribot[2] admits these as equally tenable, but gives some space to Hecker's "contrast and intermittence" theory as a third. I would add to these three groups a fourth, the "liberty" or freedom theory, first advanced by A. Penjon.[3]

It appears that Hobbes was the first to stand sponsor for the superiority theory. He expresses it as follows: "The passion of laughter is nothing else but sudden glory arising from sudden conception of some eminency in ourselves by comparison with the infirmity of others or with our own formerly." Sully,[4] in support of the theory, points out that "we laugh at all sorts of littleness, discomfitures, unworthiness and so forth, provided that they are not serious enough to excite compassion, to offend our sense of decency or evoke other incongruous feelings." Bain[5] observes that "the occasion of the ludicrous is the degradation of some person or interest possessing dignity in circumstances that excite no other strong emotion. . . . The element of the genuine comic is furnished by those dignities that from some circumstance or other do not command serious homage." The reader is referred to Prof. Bain's enumeration of the degradable quantities. Another means of degradation, according to Bergson,[6] is the mechanization of organic movements, the insertion of mechanism in life processes. A ready experiment is that of stopping one's ears to the music of the dance. The motions of the participants at once become ludicrous. The drill service of the army recruit, the divinity student making his maiden effort, the new clerk at the counter, the city chap trying to be "handy" on the farm, in fact the attempts of all novices are ludicrous because they convert organic into mechanical movements. Punch and Judy, jumping jacks, and all forms of punchinellos owe some of their oddity to their rigid and wooden movements.

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[1] Groos, Karl: Play of Man, pp. 232-237.

[2] Ribot, Theodore: Psychology of the Emotions, pp. 352-357.

[3] Penjon, A.: Le Rire et la Liberté. Revue Philosophique, pp. 113-140, Aug., 1893.

[4] Sully, James: The Human Mind, p. 150.

[5] Bain, Alexander: Emotions and the Will, pp. 257-260.

[6] Bergson, Henri: Le Rire, Essai sur la Signification du Comique, Paris, 1904.

Some of the more patent and random facts of the superiority theory are seen in the savage's laugh over a fallen foe, in the practical jokers guffaw at his victim, in the soldier's shouts of victory, in the wild uproarious shouts of the winners in many forms of political and athletic rivalry.

The theory of "contradiction" or "incongruity" has more adherents, perhaps, because the data forming the general notion is so plentiful, diversified and patent. This has led to its exploitation along seemingly different lines. Schopenhauer[1] and Kant and their commentators hold that the humorously incongruent consists in a comparison between a norm and its imperfections. For Schopenhauer the norm is a concept and the imperfection is resident in a related percept. He says: "Therefore in everything that excites laughter it must always be possible to show a conception and a particular; that is, a thing or event which certainly can be subsumed under the conception and therefore thought through it, yet in another and more predominating aspect does not belong to it at all, but is strikingly different from everything else that is thought through that conception." He thinks this is an explanation of the humor provoked by certain animal forms such as apes, kangaroos, jumping hares, etc. There is something about these creatures resembling man which leads us to subsume their forms under the conception of the human form, and starting from this we perceive their incongruity with it. Another variation of the incongruity doctrine consists in grounding the comic on a "baffled attempt" to unite incongruous parts into harmonious wholes. The baffled attempt may or may not be conscious. The conscious result is described as a "delicate, sudden surprise." Kraepelin[2] speaks of it as "an unexpected intellectual contrast which awakens in us a contention of æsthetic, ethical or logical feelings with a preponderance of pleasure." Dr. G. Stanley Hall[3] explains wit by this sort of incongruity which he thinks functions as a shock to the psychophysical organism. The common mind even speaks of being shocked by wits, jokers and punsters. Bain admits that the incongruous is ludicrous if it is degrading, while Spencer[1] urges its efficacy to produce laughter if it is descending but not necessarily degrading. Groos observes that these two theories (superiority and incongruity), are by no means exclusive the one of the other, but that they are only opposed in that each accuses the other of failure to cover all the facts. Sully and Ribot attempt to harmonize them on an evolutionary basis. The degradation theory explains the humor of the primitive mind,—the mind that gloried in the sudden sense of physical power and physical victory, while the modern mind finds enjoyment in incongruities, "in those fugitive and subtle contradictions which constitute the principal element of the comic to be caught on the wing." Groos absorbs both theories in one by considering the appreciations of the comic as a form of play grounded on the instinctive indulgence of the fighting impulse, aided and enlarged by the ideas of contrast.

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[1] Schopenhauer, Arthur: The World as Will and Idea, Vol. II, pp. 275-280. Translated by R. B. Haldane and J. Kemp. (1819.)

[2] Quoted from Theodor Lipps, Komik und Humor, p. 29. (1898.)

[3] Hall and Allin: The Psychology of Tickling, Laughing and the Comic. *Amer. Jour. of Psy.*, Vol VIII, p. 27.

Ribot calls attention to the fact that the nature of laughter (the comic) would be very incompletely known were we to confine ourselves to pure psychology. Humor is a psychophysical phenomenon writ large, of which laughter is the physical aspect. The causes of physical laughter are legion, and, as Darwin[2] has observed, extremely complex.

Spencer was the first to consider and develop a satisfactory theory of the physiology of laughter. He points out three possible channels through which nerve centres in a state of tension may discharge themselves. One group discharges into other cortical centres which have no connection with the bodily members, a second group discharges into motor centres and thereby causes muscular contractions, a third may pass on the excitement in centres which discharge into the viscera. The channels which discharge into the motor centres may cause laughter. "Laughter, then, is a form of muscular excitement" and so illustrates the general law that feeling passing a certain pitch habitually vents itself in bodily action. The movements of laughter are without purpose and aim. They yield no product. In this respect they are similar to shivering, to certain automatisms and to some forms of imitation and play. They seem admirably fitted as spill-ways for uncontrolled energy. And herein lies the explanation of why certain classes of muscles are affected first and then certain other classes. "For an overflow of nerve force undirected by any motive will manifestly take the most habitual routes, and if these do not suffice will next overflow into the less habitual ones." The organs of speech furnish and are therefore the most frequent outlets for feeling. The muscles around the mouth are small and easy to move and are the first to contract under pleasurable emotions. The mouth movements are followed in order by those of respiration, then by those of the upper limbs, and if these latter are not sufficient, the more central and fundamental muscles of the head and body are brought into action producing the "hearty laugh." This explanation of the physical aspect of laughter has, so far as I know, been generally accepted in its main features.

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[1] Spencer, Herbert: Physiology of Laughter, Essays Sci. Pol. and Spec. Vol. II, pp. 452-466. (1860.)

[2] Darwin, Charles: Expression of the Emotions, pp. 352-354. (1872.)

In 1873, thirteen years after Spencer's paper, Ewald Hecker[1] published his unique "contrast and intermittence" theory, in which the physical aspect receives due attention. On account of its interest from an historical standpoint it is here briefly presented.

1. The vasomotor nerves which regulate the calibre of the smaller arteries can be excited reflexly by afferent impulses conveyed either from the blood vessels themselves or from the end organs of the sensory nervous system.

2. Those parts of the body rich in small blood vessels containing vasomotor nerves will experience wide volumetric changes.

3. Of such parts the brain is chief. Its delicate structure, its bony case and its large volume of blood together with other fluids make it highly important that the inflow and outflow of blood be safely regulated.

4. Such regulation is accomplished in two ways, (a) by changes in the calibre and the tonus of the blood vessels as indicated in principle 1 above, (b) by the rhythmic changes in the intra-thoracic cavity due to respiratory processes.

Hecker inferred from scattered bits of experimental evidence that tickling acts as a reflex stimulus to the vasomotor nerves, which in turn narrow the calibre and increase the tonus of the cerebral blood vessels, the result of which is to force the blood from the brain cavity, thereby inducing anæmia. It is further argued that inspiration co-operates with tickling in causing anæmia while expiration checks the blood flow from the brain, thereby restoring normal blood pressure. Any device, therefore, which prolongs expiration tends to correct the anæmic condition. For Hecker laughter is such a device; it is a biological activity developed through natural selection like any other survival-value process. The laugh is a powerful reflex movement which compensates for the diminished blood pressure caused by the tickle.

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[1] Hecker, Ewald: Physiologie und Psychologie des Lachens und des Komischen, Berlin, 1873.

We are not sure, however, that an anæmic condition of the brain favors laughter; it is a mere inference. A case can be made out in favor of hyperæmia and laughter,[1] not to mention the fact that the presence of vasomotor nerves in the vessels of the cortex is still unproven.[2]

Hecker's psychical account of laughter is grounded on the physical. Laughter as a mental process consists of an accelerated contention of feeling, of a hither and thither fluctuation between the pleasant and the unpleasant in which the unpleasant aspect is neglected, that is, is unconscious. Continued contention between the two feelings produces mutual intensification until, as in the comic, "pleasure is passing over into pain and pain into pleasure." This intermittent feature of the comic is analogous to that of the physical tickle, producing a similar anæmic condition of the brain. Here again physical laughter restores the normal blood pressure by prolonging the respiratory process. The defects in this theory have been pointed out by Hoeffding[3] and Lipps,[4] by the latter at great length.

The potency of these time honored theories stubbornly to resist a Hegelian synthesis and to perpetuate the irreconcilable camps is uniquely demonstrated by Miss L. J. Martin,[5] in her paper on the "Psychology of Æsthetics." Miss Martin presented to the view of her reagents a pictorial caricature on the phrase, "Spring, gentle Spring" by Mr. Kemble, of *Life*. They were asked among other things to assign a cause for the comic in the picture. Concerning "superiority and degradation," thirty-seven of the reagents have a feeling of superiority in connection with the Kemble picture. The remaining twenty-three report themselves as having no such feeling, while a hundred and eighteen interpret it under some variety of Schopenhauer's "contrast theory."

The sections that follow consider the "Liberty" theory and its possibilities for absorbing its predecessors.

III. Humorous and Non-Humorous Stimuli.

The immensity of space, the infinitude of time, the alternation of day and night, the movements of the heavenly bodies, all rhythmical changes, never inspire humor. The same thing is apparently true of all physical, chemical, and mathematical laws, and, likewise, of the macroscopic things of earth, the waters, tidal movements, cataracts, mountains and forests, deserts and plains. According to Schütze,[1] dead things, rocks, stones, rivers, are never laughable unless human qualities are ascribed to them. Bergson declares "There is no comic outside of what is properly human." Organic life in the large, such as swift, rhythmical movements of large numbers of animals, may inspire awe and dread but never humor. The orderly expression of life processes, such as the heart beat, the mystery of sleep, self-consciousness, birth and death, give no sense of humor. There is a large group of objects, situations and actions which incite feelings of disgust, of loathing and even hatred. Of objects there may be mentioned parasites, creeping, crawling and slimy things, all filth, all skin and eye diseases; and of actions, all forms of tyranny and bullying, treachery, poltroonery, ingratitude and, according to Bain, the "entire catalogue of the vanities given by Solomon."

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[1] Stewart: Manual of Physiology, pp. 254–255.

[2] American Text Book of Physiology, p. 204.

[3] Hoeffding, Harald: Outlines of Psychology, pp. 291–292.

[4] Lipps, Theodor: Komik und Humor, pp. 9–15.

[5] Martin, L. J.: Psychology of Æsthetics, *American Journal of Phsychology*, Vol XVI, pp. 108-109.

There are a large number of objects which, so far as humor is concerned, constitute an indifferent zone. Subdued colors, gray tones, all natural forms of locomotion and movements, all common and customary occupations, all actions and events of familiar notice belong here.

By this process of elimination it appears that the conditions averse to humor are, (1) the macroscopic things of the world together with their laws, order, harmony, and rhythm, (2) those things which are inimical to life and freedom, (3) those things, largely of the social order, that have become habitual, regular in occurrence and necessary to human comfort. There are left for consideration animals and their actions, man and his actions, clothes, customs, manners, words, language and thoughts.

1. *Animals.* Small animals, like small people, are more likely to provoke humor than large ones. The bantams and games are the clowns and Don Quixotes of poultrydom, while the Plymouth Rocks and Shanghais are the prosaic members. The poodles, terriers and spaniels are the fun makers of the kennel, the St. Bernards, great Danes and bull dogs command our serious respect and sometimes more. When an animal of one class does the task common to an animal of quite a different class, it is apt to provoke humor. An ox in shafts drawing a top buggy; mules, asses or buffaloes running a race; an elephant drawing a chariot are examples. But if the animal is set

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[1] Schütze, Stephen: Versuch einer Theorie des Komischen, p. 36, 1817.

to doing a human task the humor is intensified. The inimitable Æsop, endowing animals with human craft and qualities, made this style of humor classical for all time. It appears in modern humor in the doings of Johnny Bear, in the clever tricks of Brer Rabbit and Brer Fox, and in the county fairs, charity balls, political conventions, clinics for appendicitis and the like conducted by divers species humanly socialized.

2. *Man.* Man may provoke humor by his size, especially if extremes meet. The undersized is likely to amuse, especially in his pretensions and passions. Unusual features, types of ugliness, odd shapes, and Falstaffian proportions contain humorous elements.

3. *Actions.* Mimicry and all actions of a pretentious and useless sort, and in false time and space relations may provoke humor. All mimicry is humorous, whether in the form of the puppet show, the pantomime, the burlesque or the comedy. The pursuit of uncertain pleasures,[1] idle, and sometimes serious, gallantry, premature and rustic courting readily classify with humorous actions. Useless actions of the ideomotor and absent minded type are the causes of many of the comedies of errors in every day life.[2] Examples are numerous: A young lady partially disrobed to make a toilet at the noon hour and wound up by "saying her prayers," that being her usual next step in the evening. We recall also the well-known anecdote of Newton, who carefully put his watch in the kettle, and resumed his mathematical labors with the egg on the table beside him to indicate the time. The wrong use of objects, tools and machinery often make an act humorous, for instance, an Indian purchasing a hearse for a carriage and taking his family to church in it. Awkwardness is a common type of action naturally humorous. Any action inherently serious may become humorous by occurring either out of time or out of place. Singing ahead of time, applauding alone, answering questions at the wrong time on formal occasions, an unmindful deacon removing his small coat with his overcoat and sitting down in his shirt sleeves in church, are cases in point. Hazlitt remarks, "in jocular history everybody is at angles to real life; people do precisely what they ought not to do, say what they ought not to to say, are found where they ought not to be found."[3]

4. *Clothes.* Clowns and professional fools supplement their wit, humor and mimicry by their well-known forms of dress. Johnny Bull, Uncle Sam, Santa Claus are always good-na-

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[1] Hazlitt, William: English Comic Writers, p. 14. 1819.

[2] Jastrow, Joseph: On Lapses of Consciousness, Pop. Sci. Mo., Oct., 1905.

[3] Hazlitt, W. C.: Studies in Jocular Literature, pp. 221-222.

turedly received, partly on account of their dress. Halloween, masked balls, Mardi Gras and carnivals, ancient and modern, owe much of their charming good humor to dress. It is well known that we laugh at the dress of foreigners and they at ours. "Three chimney sweeps meeting three Chinese in Lincoln's-Inn-Fields, laughed at one another till they were ready to drop down. Country people laugh at a person because they never saw him before. Any one dressed in the height of fashion or quite out of it is equally an object of ridicule." Doubtless if the centuries could rise up and view each other *en masse*, their first act would be mutual laughter at each other's clothes.

5. *Customs and Manners.* As stimulants of humor customs and manners have, perhaps, no equal. They excite it alike in the vulgar and in the cultured, in the illiterate and the learned. They appear in excesses and exaggerations and in violating time and space relations, either as innovations or as lingering too long. To appear in excess or out of time and place implies some age and stability in human institutions. Norms and standards of fashions must be formed, regularity in activities must freeze into custom and the free spirit of good fellowship and of social intercourse must become habituated to the plane of manners before the spirit of satire, wit and humor can react for or against them. This is so apparent in the literature of every people that it seems unnecessary to make special references.

6. *Words, Language and Thought.* This is the favorite tramping ground for both the humorist and his critics. Here occur the most delicate, subtle and refined specimens. It is also here that an attempt to give an adequate treatment resembles trying to bottle a fog or lasso a cloud. To make some headway, however, we are under the necessity of drawing a few distinctions. All words, language and thoughts not humorous to the speaker but so interpreted by the observer may be termed *unconscious humor* (following the lead of common usage). The humorous interpretation of unconscious humor may be called *passive humor*. All deliberate use of words, language and thoughts and the exploitation of all other hnmorous stimuli by the subject for humorous effects may be considered *active humor*. In what follows the text indicates which sort is meant.[1]

a. Concerning words it appears that their misspelling, mispronunciation, misinterpretation, forced and misusage, punning, repetition, localisms, foreign accent and even intonation endow them with a certain degree of humor. Many of the humorous classics use one or more of these methods. The writings of "Artemus Ward" and "Josh Billings" practically exhaust the possibilities of misspelling. All forms of dialect now occupy much of the humorous field of mispronunciation and misinterpretation. Dickens displays the worth of forced usage in the inimitable Pickwick. Sheridan creates Mrs. Malaprop largely by these methods. Shakespeare had the courage to pun to his own satisfaction. Dickens again has used repetition to a fine effect in several of his characters. We recall Mr. Toots, "of no consequence" and Joey Bagstock who is "devilishly sly." Provincialisms and foreign accents enter into the humor of daily life more than into that of literature. The unconscious distortion of words by the illiterate, the naïve and the pretentious adds to the quality of this sort of humor. In fact, whether the distortions are "made" or are unconscious, their humor depends on our apprehending them as unconscious. A farmer who made daily business trips to Richmond assured his neighbor that he always dined at a "first-class reservoir." A colored servant in my own home asked for a half holiday to go on a "railroad 'squashin.'" (What irony in the light of recent events!)

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[1] For a discussion of the forms of the comic, see Lipps: Komik und Humor, pp. 78-102.

b. Language, much more than customs and manners, requires a civilization of some age and stability in order to furnish both the conditions and the material for humor. George Meredith[1] has urged that a society of cultivated men and women is required wherein ideas are current and of some duration, and the perceptions quick that the humorist may be supplied with matter and an audience. "The semi-barbarism of merely giddy communities, and feverish emotional periods" create no humor. Quaintness in language, as in other things, gives a tinge of humor. A description of the table manners of a nun or of a lady of culture in modern language would be sorry business, but when Chaucer says of the nun,

> "At mete wel y-taught was she with-alle;
> She leet no morsel from her lippes falle,
> Ne wette hir fingres in hir sauce depe."

he stimulates our sense of humor. Here belongs the grave and serious style in connection with trivial and prosaic matters. Many of the failures of language to fit the thought yield humor, a common type is verbosity. In this connection I may cite the following, which is supposed to come from the Mt. Sterling, Ky., *Reporter* (Colored):

Dear Editor:

Please allow me a space in your momentous Gazette to reciprocate my gratitude to the indefategably workers of the Evergreen Baptist Church. While sitting in my studio last Friday evening greatly obsorbed in the momentous problem so called Negro problem I were interrupted by the anthem "There shall be showers of blessing" which rendered me surprisal happy. . . . . . After a general parlance I were divinely impressed to descant on the altronistic spirit that should characterize the christiandom. A sumptuous repast followed and all present shiated their gastronomic desire. Bro. Ben Mitchell distinguished himself from the rest by his implacable vorasity. May God bless the members of the Evergreen Baptist church. Many thanks. *F——— B———, Pastor.*

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[1] Meredith, George: An Essay on Comedy, p. 8, London, 1905.

Ultra slang, brusque catch words and phrases of common life may provoke humor for a short period. This field is illustrated by the monologues of Chimmie Fadden and the writings of George Ade. The speech of the excited, the irritated and the fatigued is often rendered humorous by inversion, omissions and awkward substitutions. A prospective bridegroom at the church door in consultation with his minister inquires excitedly, "Is it kistomary to cuss the bride?" Grumio answers his master, "Ah, sir, they be ready; the oats have eaten the horses." Taming of the Shrew, Act III, sc. 2.

c. What a man thinks or feels, although serious to him, may be just as much an object of humor as a situation, an awkward movement or a form of speech. The unconscious maker of humor in thought is my next neighbor. More specifically it is the illiterate, the ignorant, the inexperienced, the credulous, the skeptic, the superstitious, the over-serious, the vain and the prosaic. The humor usually appears in the attempt to deal with situations and problems somewhat beyond their ken. The ignorant and illiterate amuse by their literalisms, pretensions, evasions and superstitions. Dickens makes frequent use of this form of humor, witness Joe Gargery's pretensions at reading for Pip's benefit. Thackeray's Captain Rawdon Crawley is a fine specimen of stupid ignorance. Many superstitions are kept alive by their humorous vein.[1] Inexperience is the lot of childhood and its humor is expressed in its questions, in its wonderings and its explanations. This is abundantly verified in the literature of childhood, now occupying excessive space in current magazines. The humor of the credulous appears in a condensed form in their responses to the yarn-spinner and prank-player. The faith and works of the inventor are often ahead of his time and are therefore the butt of the common mind. Cervantes made Don Quixote the prince for all time of the over-serious, and Malvolio of Twelfth Night typifies the vain among individuals of small parts. Delivering great force into small matters, exercising much thought over petty questions, exalting trifles to the plane of the magnificent form a perennial source of humor.

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[1] Johnson, Clifton: Some New England Superstitions: New Eng. Mag., Oct., 1906.

Active-thought humor. Active-thought humor is as complex and infinite in variety as thought itself. Its consideration naturally belongs with that given to the *nature* of humor in the next section. It is taken up here for the reason that it employs a wider range of humorous stimuli than that just detailed. Perhaps the earliest form of this humor is expressed by the child in its "surprise" and peek-a-boo plays. While learning to talk it uses mimicry to good effect and a little later imitative and even original tricks are performed for amusement. From about four years old to ten original humorous drawings are made with considerable zest. As soon as they acquire some degree of familiarity with the mother tongue, guessing games, riddles, conundrums old and new, rhyming, punning and joke-making are used with greater or less frequency for humorous purposes. This period naturally connects with the drollery, clownishness and prank-playing of adolescence—a period rather barren in creative humor. Adolescence laughs much but creates no humor, so far as I can ascertain. The gravity of life is dawning and self-consciousness is too frequently on guard, thereby precluding the conditions for creating humor. Cicero[1] was the first to have extensively considered active-thought humor in adult life. He apologizes for his number of headings. "I have divided these matters into too many headings already—but in general their varieties are reducible under a few general heads; for it is by deceiving expectation, by satirizing the tempers of others, by playing humorously upon our own, by comparing a thing with something worse, by dissembling, by uttering apparent absurdities and by reproving folly that laughter is excited." Elsewhere he includes "pretended misunderstandings, wishing the impossible, uniting discordant particulars, and concealed suspicion of ridicule." Of course it is evident that the wit and the humorist employ these several means together with those I have already described, while the cartoonist adapts the same principles to the pen and brush.

IV.

THE NATURE AND ORIGIN OF HUMOR AS A MENTAL PROCESS.

Schütze in 1817 and Hazlitt in 1819 résuméd the various opinions as to the nature oi humor up to their time. The former cites some fifteen different authorities and views.

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[1] Cicero: Oratory and Orators, Bonn's ed., p. 303.

Schopenhauer in 1819 made a decided contribution in that he attempted an exact description of the mental processes involved. Since then the nature of the mental process and its physiological basis have been the main points of discussion. Schütze, Hoeffding and Sully call attention to the sense of freedom involved. Penjon in 1893 describes at some length the relation of this sense to humor.

I have already pointed out that the appreciation of law, of order, of harmony and of those things that are inimical to life and freedom begets a sober mental attitude, the intensity of which varies with the weightiness of the matter and the issues involved. Now if when dealing with such matters, the thinking process continues organized and controlled and progresses toward an end, it is termed rational. But if the mental tension exceeds the capacity for controlled thinking, brought on by the sudden triumph of wrong and evil values, disruption of the thinking process at once ensues accompanied by an unpleasant emotion ranging from mild disappointment to the tragic: if, on the contrary, the disruption is caused by the sudden triumph of good values, a pleasant emotion results. In either case organized and rational processes give way to those of an uncontrolled and emotional sort. The mental stream has had its banks torn away and its forward movement stopped, voluntary movements are replaced by hereditary. In the more intense form a reversion to primitive conditions may occur; for we then do and say things that may shame us in our sober moments. Now the humorous process occurs in just such a disrupted consciousness at the triumph of good and pleasurable values preceded by a mental tension similar to, but not always equal to, that preceding emotions. The common and quiet forms of humor usually occur in a consciousness that has been running at its usual strength and depth, sufficiently organized to command the situation, assume a definite form and take on a certain strength of surface tension. (The term, "surface tension," simply extends the water metaphors of psychology in a logical direction. I use it to indicate the impervious condition of consciousness formed in any attentive state, the strength of the surface tension being in direct proportion to the intensity of attention.) The function of the humorous stimulus consists in cutting the surface tension, in taking the hide off of consciousness as it were and in breaking up, in part only, its organization, which is at once followed by the humorous feeling, the next wave in the stream of consciousness. The cognitive elements in the humor process consists, (1) of the perception of the stimulus, (2) the sense of freedom. Each of these cognitive elements is suffused by pleasant affective elements which constitute in their totality the unique and dominant humor tone. The uniqueness of the humor tone is the crux of the matter. The mental tension preceding the humor process is not the differentia, for that precedes any and all emotional states, yet it is an essential condition. The clue lies in the nature of the humor stimulus and the relation sustained to it by the individual. This stimulus belongs to an order of knowledge, whose laws, uniformities, manners and customs have arisen since the human mind has attained its present estate. Contrast with the humorous stimuli the non-humorous and it appears, humanly speaking, that the latter have always existed. The heavens, the laws of matter, cosmic forces of whatever sort were in full swing when human consciousness dawned, their operation has participated in mental evolution and to that extent has impressed law and order upon it. Therefore, when we are engaged with these things, sober thinking, pleasant or unpleasant emotions are the outcome, but never humor. "The spirit of mind in infancy may be through and through playful, but as it unfolds and develops in a world of order and law, it comes to operate in an orderly fashion. We see nature in terms of law and order, that is in terms of science."[1] But it will be noticed that the humorous stimuli are departures, exaggerations, even violations of the laws, uniformities, concepts and what not that have evolved out of man's experience. The significant fact for humor is that these departures, exaggerations, etc., do not disturb the recognized values of good and evil. The mind maintains all the while a disinterested attitude toward the object of its activity. We seek neither to correct nor further exaggerate the departure from the norm. It is time to feel and not to act. We enter into æsthetic rather than practical relations with the object of our humor; should we seek the practical the humor at once ceases, issuing, perhaps, in bitterness or joy, sarcasm or flattery, indignation or admiration.[2] Penjon writing upon this point says: "If one separates, as must be done, the causes which too easily deform the comic and make of it an emotion of wickedness or bitterness, the comic emotion will appear purely disinterested. I mean by this that the object or the event which is the occasion of the comic excludes every idea of loss or of profit, that it makes us conceive neither hope nor fear and seems to us at the same time neither advantageous nor harmful to any one; it is worth in itself what it is worth without adding to our idea of it any consideration of end or ideal." The humorous process, then, like play, is its own end and justification. The kinship between humor and play not only suggests relationships between humor and freedom, which Penjon[1] has so well worked out, and between humor and æsthetics long ago indicated by Kant and recently treated by Lipps, but that mental activities long interpreted as play should be credited to humor. I have already indicated the survival-value of humor for superstition ; it doubtless performs a similar and larger function for play. It is suggested that a number of the so called plays of the higher mental processes named by Groos[2] are by nature humorous, the humor being expressed in forms of play. Humor, then, is an end in itself and has no practical interest in its object. *This fact constitutes its first differentia.*

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[1] Penjon, A.: *Ibid.*, p. 117.

[2] For subjective proof of this read "Polly Baker's Defense" by Benjamin Franklin, also Dickens's Satire on American life in Martin Chuzzlewit.

I have already indicated that the sense of freedom is a constituent element in the humor process. Its consideration is next in order. To that end I submit some of the evidence as it had formed in my own mind before meeting with Penjon's extended account.

The family and guests are seated about the fireside enjoying the moments of silence. The only light is that of the glowing embers. A smouldering bit of bark suddenly flashes up and a smile plays over the faces of the silent group. The stroke of a sweet-toned clock or a sneeze or the dropping and rolling of a sewing thimble or of a ball of yarn produce under similar conditions the same effect. A group of boys are seated on the bank of a bathing pond apparently gazing at the water's glassy surface. Suddenly it is broken by huge drops of rain out of an apparently cloudless sky. The boys smile. The humor response in such cases is weak and simple. At such times consciousness is damped down to dreamy, pleasant processes under lax attention, and the mild humor results from the sudden, delicate, harmless stimulus piercing its surface tension, disrupting its feeble structure and thereby permitting it to move on in a more free and spontaneous fashion. But let the surface tension and structure of consciousness become toughened, cramped and tense by responding to routine or grave or hard conditions, and objects of little or no inherent humor will become excruciatingly humorous. ''Snickerin' at nothin' '' in the school room, giggling before strangers and company, especially at the table, the increasing intensity of annoying return waves of humor on solemn occasions are cases in point. Bain observes that in a court of justice or in an assembly of ordinary gravity a trifling incident causes laughter, and that the young are the greatest sufferers by the imposition of gravity and are the most disposed to break free from it. They entertain a mock solemnity for the intense delight of rebounding from it, just as they toil up to the top of an eminence for the sake of the downward run. Members of college glee clubs inform me that they see humor in everything while on their vacation musical tours. Darwin records that the German soldiers before the siege of Paris, after strong excitement from exposure to extreme danger, were particularly apt to burst into laughter at the smallest joke. I have received testimony from eye witnesses of the San Francisco earthquake to the effect that for several days following the shock not only laughter but good nature and humor overflowed on slight provocation. The history of humorous literature discloses the fact that it is most prolific in those crises and changes in human affairs at which the consciousness of freedom breaks out. Martin[1] tells us that the parody was first introduced during the performance of Greek tragedies to relieve the audience from the intense mental strain. In the severe atmosphere of the king's court the court fool was an important adjunct. In reality his was the freest personality of the group, the king not excepted. King Lear and his fool furnish a most striking example in literature.

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[1] Penjon : *Ibid.*, p. 116.

[2] Groos, Karl : *Ibid.*, pp. 122-169.

These considerations indicate an intimate kinship between the humor process and the sense of freedom. The real connection becomes apparent when the nature of the stimulus is taken into account. It has already been shown that the stimulus perverts and breaks up the mechanism and order about us. It appears as the only objective fact in our experience that dares to defy the world order with impunity, that can violate ruthlessly, without pain and without apology, the manifold human contrivances, social customs and relationships and thereby not only creates the sense of freedom, but also assures us that we may temporarily escape from the uniformities and mechanisms of life. The sense of freedom, once born through these temporary escapes from uniformity, has been objectified into the so-called principle of freedom. Any means that will lift the veil of law and social order and reveal freedom as an abiding reality, will produce pleasure. Play, art and the humor stimulus are such means. Play performs this function especially for the young, art for the trained and educated, but humor ministers impartially to all ages and classes. By its revelation of freedom, abundance of life, springs of spontaneity and possibilities of progress are made more manifest. So long as the present order of things is taken for granted, so long as present ways and laws are accepted as the only ways and laws, change and progress if any is accidental. The humor stimulus rends the veil of uniformities and bids us look behind the scene where luck, chance, spontaneity and life operate. It is the uncertainties and not the certainties of life that give it zest and charm. Let the outcome of a game or contest become a "dead sure thing" and interest will flee away. We are rather chary of an over-scientific game in which luck and chance are supplanted by rigid rules. The humor stimulus gives glimpses of the world of uncertainties, of spontaneities and of life, and in so doing creates the sense of freedom of which the sense of humor is the obverse side. The sense of freedom is the second differentia, therefore, of humor.

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[1] Martin, A. S.: Parody, p. 1.

The failure to see that the sense of freedom is a constituent part of the sense of humor is doubtless responsible for the "superiority" and "degradation" theories. The sense of power yields pleasure but not humor. The sense of power is wrapt up with obligations, practical interests and relationships, the humor stimulus does not make us aware of power. Incongruity, descending or otherwise, all disorders of time and space relations in our actions, customs and language, all mechanized living movements, all deliberate manipulation of the humor stimuli are only humorous when they excite the sense of freedom. If there is any genuine humor in the shouts of the victor, it is due to the sudden sense of freedom arising from the removal of the obstructions to victory. It would then appear that the multiplicity of humor theories may be resolved into the freedom theory. The theories hitherto advanced have been more a classification of humorous stimuli than explanations of humor as a mental process.

Origin. A cross section of our mental life shows first an aspect composed of hereditary factors (unlearned reactions), second a well defined aspect of acquired factors (learned reactions) composed of what the individual does for himself and what is done for him, and third an ill defined aspect that permeates the other two and in addition enjoys in part a separate existence of its own made up of unmechanized and elementary mental factors. One readily recognizes the second aspect as intelligence. Prof. Royce[1] calls it "docility." It may be termed mechanized mind in that it represents mind reduced to law and habit. Getting on in the world is dependent to a degree on a certain quantum of mechanized mind. Common usage employs such terms as habit, adjustment, adaptation, education, to designate such an equipment. Several processes are involved in its production, such as imitation, learning by "trial and error" and by "understanding." Of these methods those that make the most use of voluntary attention are the quickest in results and the most extravagant with mental energy; here it is that mental tension reaches its highest pitch Relief comes in a variety of well known ways, humor, perhaps being the most unique of the lot from the fact that it accomplishes its purpose with the least expenditure of energy and a a time, too, when the individual can ill afford to make sacrifices in the interest of recreation. Considering, then, the nature of humor as a mental process, and the nature of its stimulus, together with the conditions under which it appears, it seems highly probable that it emerged as a distinctive process from the states of *inattentive freedom* immediately preceded by states of *necessary attention*. It should be mentioned in this connection, that even with the sense of humor as a part of our conscious mechanism, its operation is decidedly influenced by the composition and organization of consciousness as affected by preoccupation, mood, temperament, beliefs and ideals. Miss Martin invaded this subtle field with experimental methods and came out with results well worthy of record.[1]

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[1] Royce, Josiah: Outlines of Psychology, p. 38.

V.

FUNCTIONS OF HUMOR.

The psychical function of humor is to delicately cut the surface tension of consciousness and to increase the pliancy of its structure to the end that it may proceed on a new and strengthened basis. It "spells" the mind on an up-hill pull. Perhaps its largest function is to detach us from our world of good and evil, of loss and gain and enable us to see it in proper perspective. It frees us from vanity on the one hand and from pessimism on the other by keeping us larger than what we do and greater than what can happen to us.

The physiological function appears to be common knowledge, for certainly its supposed influence on adipose tissue has passed into a proverb. Kant cherished the belief that laughter had a beneficent effect on our digestive organs even down to our very entrails. Hecker advocates the idea that it relieves the anæmia of the brain induced by tickling.

Its biological function, in my judgment, is far more unique in mental economy than is its nature as a process. I have already called attention to the unmechanized aspect of mind, a matter more readily believed than easily proven. To adduce adequate evidence of the magnitude and the importance of this aspect of mind over the mechanized and hereditary portions would lead us too far afield. For the sake of a better appreciation of our problem, however, a few considerations on the point seem worth while. First, we register our belief in its existence by such expressions as mind growth, naïveté, self-activity, spontaneity, individual variation, genius and "mental initiative" (introduced by Prof. Royce), and by more remote terms like open mind, simple mind, youthful mind, unprejudiced mind. Second, many students of mind express themselves to the same effect. President G. Stanley Hall states that surely mind is as old as the body and certainly more complex: any lesser conception would indeed be a mean one. Prof. Shaler[1] expresses this view so clearly that I quote his words: "One of the results of the marvellously swift, absolutely free development of man's spirit is that there has as yet been insufficient time for it to become organized as are the conditions of the body, working in the instinctive manner in which the lower species do their complicated work through the fore-determined mental processes we term instincts. There are always gauges and standards for the endeavors in the mind as there are in the bodily frame. With us, however, all kinds of thinking are still a hurly-burly, a confusion, to which time and culture may possibly bring something like the order it has in the lower life, but which probably is as ever to remain in its present uncontrolled state." Third, biologists are generally agreed that the hand, the vocal organs and the cerebral cortex have undeveloped capacities far beyond present realization. Their possibilities are as yet unknown, and with the cortex its capacity appears to be infinite with only a small portion reduced to law and order. If we can so confidently assert unlimited capacity of these physical structures, then any lesser conception of mind is indeed an untenable one. It does not yet appear what we shall be, but there is general agreement that the immediate path of evolution will be spiritual rather than physical. And if spiritual, it must be confined to the free portion of mind, to those parts not yet close yoked to matter and frozen into habit. Of course there is universal consent that the mind should be mechanized to the extent of the needs of common life, of routine business, of the alphabets of learning and of the elements of culture, but anything beyond these points is inimical to both individual development and to racial evolution. Influences that tend to check mechanization and to incline the mind to grapple with the new and with the ideal prolong the possibilities of spiritual development. Humor and play are two such processes, with the honors in favor of humor. It stands guard at the dividing line between free and mechanized mind, to check mechanization and to preserve and fan the sparks of genius. And, like play, it keeps the individual young, projects the best in youth into adult life, sets metes and bounds to "docility'' and prevents the mental life of the race from hardening into instinctive and hereditary forces.

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[1] Martin, L. J.: *Ibid.*, p. 64.

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[1] Shaler, N. S.: The Measure of Greatness, *Atlantic Monthly*, Dec., 1906, p. 751.

Humor is *par excellence* a social mental process, a distinctiv feature of the social consciousness. It is, therefore, not completely described until that aspect is taken into account. Then too, its bearing on creative art and the uses made of it in ou social, political, commercial and industrial life call for mor detailed attention than this paper can undertake.

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I wish here to express my thanks and indebtedness to Dr. Louis N Wilson, Librarian of Clark University, for placing the literature of th subject at my command, without which the present paper would hav been impossible; and also to my wife for helpful criticism, and assistance in examining and arranging the literature. The bibliograph appended contains a list of the works examined in the preparation this paper.

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ANALOGY IN THE LANGUAGES OF PRIMITIVE PEOPLES.

By Alexander F. Chamberlain.

One of the ways in which names are given to new and unfamiliar objects and things is from analogy with the old and familiar. An inspection of the vocabulary of our own English language shows how many times this has happened with us,—in various Aryan tongues "cat," "tiger," "lion," "cow," "dog," etc., have often given rise, through analogy, to other appellations of animals. And with primitive peoples this process is naturally more in evidence. This procedure is interesting psychologically both in relation to the laws of association and by reason of the fact that it occurs also so frequently in the language of children, to whom the naming of the unknown by analogy with the known seems to be as natural as it is to the savage and the barbarian. The examples here given are drawn from the language of the American Indians, whose value for the psychologist is just beginning to be appreciated.

Apple. This fruit seems to have taken the fancy of the Indians, and there are consequently many very interesting names for it, *e. g.*:

1. *Rose-hip.* The Kootenay name for apple is *gollwa*, which properly designates the "hip" of the wild prairie rose (*Rosa pisocarpa*); the same word is now used also to designate apricot, peach, pear, tomato, etc. The English term "rose-apple" for rose-hip is an interesting instance of like analogy.

2. *Cranberry.* The Micmac *wenjoosoon* signifies, literally, "French (*wenjoo*) cranberry (*soon*)." The analogy between the cranberry and the apple is not at all confined to the American Indian. While on Cape Cod, a few years ago, the writer heard a lady, who had never seen cranberries growing before, exclaim, "Do you know what these cunning little apples are?"

Beet. In several Indian languages, the beet, with which the natives became acquainted after having known the turnip, was named after the latter. Thus in Ojibwa, *miskotchis*, "beet," signifies, literally, "red turnip."

Bottle. The word for bottle in the language of the Massachusetts (Natick) Indians, *quonoasq*, designates properly a "gourd." But in translating Matt. 9:17, Eliot used, for "in new bottles," *wuskishquadt*, derived from the radical *wiskq*, "pot, dish, or vessel."

Buckwheat. The Micmac name for buckwheat, *sooomancheejul*, signifies, literally, "little beechnuts," from the resemblance in shape.

Candy. In Micmac candy is called *upkoo*, which properly signifies "chewing gum," and, before that, "pitch" (of a conifer). Here the "chewing" has determined the name. The Kootenay Indians, of British Columbia, from another point of view, call candy *gaktletl k'koktci*, "variegated (striped) sugar."

Chocolate. This drink, itself of American Indian (ancient Mexican) origin, was, however, unknown to the northern tribes until introduced by the whites. Says Bishop Bompas (Dioc. of Mackenzie River, Lond., 1888, p. 97): "Chocolate is a favorite beverage with the sick, where it can be obtained, and it is looked upon as a medicine. The Indians universally give it the name of *ox blood*, because it was mistaken by them for the blood of the musk ox, when they first saw it used by the whites." The analogy is, perhaps, between the chocolate in cakes, or before using, and the coagulated or frozen blood of the musk-ox. In Ojibwa chocolate is called *miskwabo*, "red liquid." In Nipissing, Cuoq (Lex. Alg., Montreal, 1886, p. 231) defines *miskwabo* as "mélange de sang, de graisse et de farine,"—from this, as in Ojibwa, the name passed over to chocolate. In these Algonkian tongues there is a folk-thought and probably also an etymological identity between *misko* or *miskwa*, "red," and *miskwi*, "blood."

Cinnamon. The Ojibwa name for cinnamon is *miskwanagak*, literally "red-cedar bark," the analogy being determined by the crude form in which this spice first came to the attention of these Indians.

Cock of gun. The Ojibwa name is *obwâmens*, "little thigh," with which corresponds exactly in meaning the Micmac *aboogwŏkŭjeech*.

Cork. In Ojibwa and Nipissing cork is called *wajashkwedo*, which term properly designates "fungus or mushroom, on trees or in the ground." The analogy with fungus or mushroom is a very natural one.

Elephant. The elephant, as a captive in the parks and "zoos" of American cities, and as a prominent figure in circuses, became known to members of many American Indian tribes,—also by means of pictures in books, etc. The names conferred upon this animal indicate the diversity of the analogies suggested to the Indian mind. Mooney (Myths of the Cherokee, p. 265) tells us that the Cherokee call the elephant *kămámă útănâ*, "great butterfly," by reason of "the supposed resemblance of its long trunk and flapping ears to the proboscis and wings of that insect." One of the Ojibwa names for the elephant is *kítshi kokūsh*, "great pig." The Micmac term for elephant, *bâstogobăĭit*, signifies "a sea cow."

Fig. The Ojibwa name for fig is *gítshi shōmin*, "big raisin" (*shōmin* originally signified grape, then raisin). In Micmac the fig is called *tāpatatweemunŭl*, literally "potato fruit," or "potato balls," from the supposed resemblance.

Goat. The Micmac word for goat, *wenjooekuleboo*, signifies, literally, "French (*i. e.*, foreign) caribou."

Haycock. The Micmac word for haycock, *wees*, properly signifies "muskrat house," from the analogy of the shape.

Hinge. The literal meaning of the Micmac word for hinge, *memegech*, is "butterfly," from the resemblance in shape of the outspread hinge to the butterfly with wings open.

Horse. The horse was extinct in North America long before the coming of the Europeans of the Columbian discovery, by whom it was reintroduced into this continent, and from whom, directly or indirectly, all Indian tribes possessing or having possessed it, have received this animal. Many aboriginal peoples, who did not adopt a foreign name for the strange animal, or create a new name by observing and describing, in the appellation given it, some of its peculiarities as they perceived them, named it from analogy with other creatures of their environment, which they thought it in some way or other resembled. Often the horse was named from analogy with the dog. Thus the Dakota *sunka wakan* means "mysterious (sacred) dog;" the Blackfoot *ponokamita* and the Kootenay *kitlkatlahatltsin* both signify "elk dog;" the Cree *mistatim* means "big dog." Another Indian tribe, the Shawnee, called the horse *mishewe*, "elk." The use of the dog by Indians as a beast of transportation probably led to the naming of the horse so often after it. The Dakota name indicates that the horse was looked upon sometimes as an animal of mystery, which fact also may have had to do with his having been named also after the elk, an animal that had several mysterious attributes among some Indian tribes, and was not infrequently a "medicine" animal, as was likewise the dog.

Lion. In many parts of North America the large, foreign Felidæ were named after the native animals they were thought to resemble. Thus the Ojibwa word for lion *mishibishi*, signifies, literally, "large wildcat or lynx" (*Lynx canadensis*); and in the Nipissing dialect *mishipishi* is applied to the lion, tiger, panther, leopard, etc.

Nutmeg. The Ojibwa word for nutmeg, *gitshi gawissakang*, signifies "big pepper." Possibly both shape and taste have shared in this designation.

Orange. This exotic fruit was often named by the Indians from analogy with already existing fruits. The Kootenay, living beyond the Rocky Mountains, and the Micmacs, dwelling on the shores of the Atlantic, speaking absolutely unrelated languages, both named it after the hip of the wild rose. Kootenay *gowitlka gotlwa*, "orange," and Micmac *chikchowwegunech*, signify, the one, "large rose-hip," the other, "rose-hip."

Peacock. The Ojibwa word for peacock, *sasega misisse*, signifies, literally, "splendid (ornamented) turkey;" and another Ojibwa name is *oshawa misisse*, "green turkey."

Peach. One Ojibwa name for the peach is *mishipagasan*, "big plum," in analogy with the plums, cultivated and wild. A Kootenay term for peach is *aqkeitlmak*, which properly designated the fruit of the wild choke-cherry (*Prunus demissa*), the stone or pit of the fruit giving rise to the name.

Pear. The Micmac word for pear, *majeokteliguncheech*, means "little arrowhead," the name being given from the analogy in shape between the fruit and the blunt-headed arrows used for certain purposes by these Indians.

Pig. The Narragansett Indians called the European hog *ockqutchaun*, *i. e.*, "groundhog" (*Arctomys monax*). Concerning this word Roger William says, "a wild beast of a reddish hair, about the bigness of a pig, and rooting like a pig, from whence they give the name to all our swine." Trumbull derives the name (Natick Dict., p. 101) from âgushan, "he burrows," and makes the word cognate with modern Algonkian terms for "pig," Abnaki *agaskw*, Lenapé *goschgosch*, Micmac *koolkwes*, Ojibwa *kokush*, Cree *kokus*, etc. It has been said, however, that the *kokus* words are onomatopœic and refer to the habit of feeding of this animal.

Putty. The Ojibwa word for putty, *wassetchiganipigiw*, signifies, literally, "window pitch," while the Cree *wabamonabisko-passakwahigan* means "window glue."

Turkey. Although the wild turkey is native to Central America and Mexico, and before the advent of the Europeans was common from Florida to the Great Lakes, over the eastern half of the country, it, like the tame bird, was foreign to certain tribes of the Northwest, etc. Thus the Kootenay name for the turkey is *gowitlka t'ankuts*, literally "big grouse," the term being created from analogy with the ruffed grouse (*t'ankuts*). In like manner, the Blackfoot word for turkey, *omuxiketukki*, signifies, literally, "big prairie chicken."

Turnip. The Micmac word for turnip, *wenjooesugebun*, signifies, literally, French (foreign) ground-nut." The "ground-nut (*sugebun*)" was the *Apios tuberosa*, etc.

Vinegar. The Ojibwa word for vinegar, *shiwabo*, signifies, literally, "sour liquid," which is also the meaning of several other Indian names. The Kootenay term for vinegar, *kowistlahane*, means "it is sour."

Whiskey. The many ways in which intoxicating liquors of

European origin were thrust upon the Indians of North America, and the amount of these they were induced to consume, have evidently made a great impression on their minds, if one may judge from the many names applied to it by them, ranging from "fire-water" to "new milk." The Kootenay Indians, *e. g.*, have these names for whiskey: *Nipika wuo*, "spirit water;" *nolluqane wuo*, "the water of the stranger;" *suyapi wuo*, "white man's water;" *wuo*, "water" (*i. e.*, "*the* water"). The Ojibwa *ishkote wabo*, "fire liquor," and its relatives in other Algonkian dialects, indicate the source of the colloquial "fire water." The Micmac *booktawich* is derived from *bookta*, "fire."

The examples cited above of analogical naming will serve to indicate the wealth of psychological material in the field of research. There are abundant data for an extended monograph dealing with the primitive tongues of America alone, to say nothing of those of other regions of the globe. It is curious to find an elephant and a hinge both termed a "butterfly." And "oxblood" for chocolate, "potato balls" for figs, "arrowhead" for pear, etc., reveal interesting turns of the aboriginal mind. For comparison with the corresponding phenomena of the language of children a dictionary of analogy-names in the speech of primitive people would be of great value. On this the present writer has made a beginning.

THE RACCOON: A STUDY IN ANIMAL INTELLIGENCE.

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INTRODUCTION.

The aim of comparative psychology is, in the end, a classification of minds according to the kind and complexity of their thinking. As the criterion for this thinking we are of course confined, in the case of animals, to what they do. We study animal behavior. We can observe what the animal does spontaneously in the wild (or in captivity) and how he reacts in experimental conditions. And nowhere is it more essential that these two sorts of observation should go hand in hand. No experimenter can feel perfectly sure that his conditions of experimentation are such as to show the animal at his best without a most careful study of what the animal does or does not do in freedom and of its own accord; and no student of animals in the wild can be sure that his interpretation of what he sees is the true one unless the animal can be led to do the same sort of thing under fully known and controlled conditions. As Prof. Jastrow (17) remarks, "we can judge what animals think only by what they do, yet what they really do may be wholly different from what they apparently do."

Trustworthy observations of the life of raccoons in the wild are not easy to obtain, because, perhaps, of their nocturnal habits; and little that is definite and of psychological interest is to be found in literature. Such personal observations as the writer has been able to make upon a dozen or more of these animals in captivity will presently be described. The experimental studies which follow have been for the most part of the general type of those made by Thorndike, Kinnaman, and others, and furnish, it is believed, a tolerable basis for the relative ranking of the raccoon with reference to the animals studied by these experimenters.

It is perhaps fair to state, also, that the present study was begun in the early part of the year 1905, and was complete in its experimental part before the appearance of Prof. Cole's study of the same animal in the May number of the *Journal of Comparative Neurology and Psychology* for the current year. The two papers are nevertheless to a considerable extent supplementary of each other, and an effort has therefore been made to correlate the results and to point out differences.

ZOÖLOGY.

Geographical range and taxonomic relations. The raccoon inhabits the southern parts of the fur-bearing regions. Lewis and Clarke found the animal at the mouth of the Columbia River. The Hudson Bay Company purchased skins as far north as the Red River, latitude 50°. Dixon and Partlock obtained skins from the natives of Cook's River, latitude 60°. On the Atlantic coast, Newfoundland is the northern limit. The animal ranges southward over nearly all of North America and South America as far as Paraguay.

Matthew and Wortman from the latest study of tertiary remains place the immediate ancestor of the raccoon in the Oligocene period, and contrary to common opinion, make its affinity closer for the *Canidæ* than the *Ursidæ*.

Something of its general zoölogical kinship is shown by the following summary of M. Watson's (37) study of the placentation of the raccoon:

1. *Procyon* agrees with all the other Carnivora in which the organ has hitherto been examined in the possession of a zonary or annular placenta, and also in the mode of interlocking of the fœtal and maternal portions of the placenta, and in the consequent deciduate character of that organ.

2. *Procyon* agrees with all the members of the plantigrade section, at the same time that it differs from those composing both the digitigrade and pinniped sections of the Carnivora, inasmuch as at one spot the placenta presents a gap or deficiency, at which spot the placental structure is imperfect.

3. *Procyon* agrees with *Canis* at the same time that it differs from *Felis* in the absence of a continuous layer of *decidua serotina* from the uterine surface of the detached placenta.

4. *Procyon* differs from every other carnivöre, in the possession of placental vessels possessed of a structure hitherto only met with in the placenta of *Cholæpus Hoffmanni* among the Edentates.

5. *Procyon* differs from every other carnivöre, in the non-possession of an umbilical vesicle.

6. *Procyon* differs from all carnivores of which the young have been hitherto examined, inasmuch as the fœtus is provided with a supernumerary cuticle or epitrichium, a structure which has only been met with in the young of certain members of other mammalian groups.

The length of the period of gestation is not accurately known. Its culmination varies through April and May. The mother gives birth to 4-6, usually in a hollow tree. In the Berlin Thiergarten in the spring of 1871, a mother brought forth five young. The young have been reared in captivity in America in several instances. At birth the young are small, about the size of a half grown rat. It is asserted that they are born blind like some other Carnivora.

GENERAL OBSERVATIONS.

In popular opinion, both among the Indians and whites, the raccoon has a high reputation for cunning and adroitness. Reports of the Bureau of American Ethnology (6, 7) have preserved for us many of the Indian folk-stories, in which the raccoon figures either as the chief actor of the story or else the subject of a parallel story in which the doings of some other animal of like cunning have been transferred to it. All these stories from whatever source have but one theme, namely, the subtility and cunning of the animal, which a short acquaintance will render as clearly apparent as it ever could have been to the Aborigines.

Food. Omniverousness has apparently had much to do with the development of the raccoon's hunting habits. He plunders the farmer's cornfield, he lifts the eggs from the woodpecker's nest, he gathers many kinds of nuts, grubs for roots, digs out turtles and frogs, goes berrying in the proper season, robs the chicken roosts, is a good fisherman and clam digger. All these and a hundred other activities combine to show the animal's great adaptability in the securing of a varied dietary. Confinement confirms the characteristics manifested in the wild. The raccoon seldom sulks when the table is spread. He never questions the cooking. He eats a good meal, and, given thirty minutes for refreshment in sleep, is ready for another as bountiful as the first. Fruit, bread, jellies, honey, sweetmeats, fish, molluscs, small crustaceans, rodents, and some green vegetables give sufficient variety for the selection of proper foods for feeding in captivity. Raccoons, however, adapt themselves to some one article of food, and as soon change to any other which may be forthcoming. I have fed them entirely on dog biscuit, then put them on a meat diet, changed to a varied diet, and then returned to the dog biscuit without much apparent loss of appetite, but the animal thrives best on a mixed diet. The particular conditions of feeding during experimentation will be spoken of later. (p.463.)

Physical Characters. The mobility of the fingers, with the sharp and partly retractile claws, which fit the hand for climbing as well as for other purposes; the bushy tail present as a balancing organ, and often slightly prehensile; the broad tuberculate molars, not thin and with cutting edges as in the purely cat tribe; the pointed and double edged canines; are all indicative of the physical resourcefulness of this animal, as well as its ability to hold its own against all comers of equal size. The raccoon climbs the bare steam pipes in the laboratory with as much ease as though they were the trees of the forest. He travels along the electric wire at the ceiling as if it were the branch of a tree and he on the under side of it. The prehensile and balancing characters of the tail appear whenever the animal attempts to climb, or stand on the hind feet in a place where the footing is uncertain, or when it is climbing a vertical surface, in which case the tail is used to steady the body in much the same manner that a woodpecker uses his while chipping the bark of the tree.

Significance of the Name Lotor (The Washer). The nicknames which the raccoon has received are closely associated with his observed habits in the wild. The Indian called him "the scratcher," "the oyster-eater." In Venezuela the Spanish local name is "Fox, wash thy hand." The name "washer" comes from the animal's habit of washing its food. Beckmann gives an account of one which used to amuse himself by washing various odds and ends in a bucket of water. "An old pot handle, a snail shell, or anything of the sort would do, but the thing he loved the best was an empty bottle. Clasping it in his forepaws he would waddle slowly to the bucket with the bottle clasped close to his breast and then roll it and rinse it in the water. If any one ventured to disturb him, he was furious and threw himself upon his back, clinging so tightly to his beloved bottle that he could be lifted by it." *The Journal of Comparative Neurology*, Dec., 1892, p. 157, cites the case of a tame raccoon which was accustomed to carry his food across his cage to wash it, and if the particles were too small, so that he ate them up on the way to the water pan, he nevertheless continued to wash with nothing in his paws. Later, however, he failed to carry the smaller particles.[1]

The reason for the washing of the food has been variously assigned. By some it is attributed to a lack of saliva. Others say that since the raccoon obtains his food largely from the water, the washing must mean that the animal convinces itself by the washing that it has made a fresh catch. Still others think that the animal washes its food merely to clean or soften it, though the process turns out to be anything but a cleanly one with coons in captivity. Each of these explanations may and probably does have an inkling of the truth, but neither singly nor together, do they make clear the origin or the purpose of this interesting and puzzling, though long observed, habit. If water is not provided, the raccoon takes the food to the dry basin and there repeats his ancestral washing. A basin and water lacking, he sits on his haunches and rubs the food between his dry paws. If the key to the problem lies somewhere back in the long list of ancestral traits we may hope that some animal under observation will yet offer us the proper solution.

Dexterity in the use of paws. The cleverness with which the raccoon uses his paws is seen in the facility with which it will catch insects in their flight, an amusement which appeals to the young ones, and also in the marked ability to pick up minute morsels of food and carry them to its mouth. Ainsworth Davis (11) remarks that, "these peculiar powers of manipulation as regards the organs of prehension, by which the food is seized and carried to the mouth, are indicative of intelligence above the average in the animals in which they manifest themselves."

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[1] Audubon remarks that he never saw his tame coon wash its food. In view of this most characteristic habit escaping the notice of the great naturalist, we can but remark, in passing, on the incompleteness of data which any single student may hope to secure from his own observations alone.

This dexterity is, however, in large degree a matter of practice. When raccoons first come in from the wild, and have gotten accustomed to take food from the keeper's hand, the paws are moved simultaneously to secure the morsel. Perfect co-ordination as to direction, in the movements either of both paws, or a single paw, is lacking. Through practice, however, the animals soon acquire the ability to use each fore paw independently, and are finally able to use one paw with greater quickness and accuracy than they formerly used the two. Burk's (8) exposition of the development of hand movements in the child would seem to throw light on this observation, for, as he remarks, "the progress in evolution of hand movements in the biologic scale has been from extreme fewness in number to infinite variety, from simplicity to complexity, from clumsy inaccuracy to precision, from simultaneous associations to those which constitute long series in sequence, from the general to the specialized. Féré shows in one of his works that the ability to move one hand without the other is small among the imbecile class. This tendency of the imbecile class to simultaneous movements suggests atavism since simultaniety is the dominating law of the fundamental movements and succession is the human characteristic of accessory movements. Simultaneous movements of the hand suggests reversion to the conditions when the hand was a fore limb." If, then, these conclusions are true we may here also find some justification for placing the raccoon above the average in the scale of intelligence.

Curiosity. While a perfected hand is without doubt a true index of intelligence, it is at best a physical and indirect one. A much more immediate and essential symptom is the exhibition of lively curiosity—in psychological terms, spontaneous attention and the instinct to investigate. Without it active intelligence is out of the question. The raccoon is an animal which displays this quality in a high degree. Its curiosity contributes to the development of its intelligence while at the same time it often becomes a source of serious lesson teaching. The animal would scarcely fall into the lure of the baited trap unless led on by its curiosity when once the scent of possible food has aroused it. The trapper takes advantage of this when he wraps a piece of tin foil around the trencher of the trap, which is then placed two or three inches under water, where it ripples a little, covering all but the foil with leaves or grass. The glittering foil prompts the coon to investigate.

Sometimes the raccoon manifests a knavery which leads to a just punishment. Groos (16) quotes from Beckmann the following account of a tame raccoon which was especially attached to a badger in the same enclosure. "On hot days the badger was accustomed to take his nap in the open air under the shade of an alder. Then the mischievous coon found his opportunity, but as he feared the badger's bite he carefully kept his distance, satisfying himself with touching his victim softly in the rear at intervals. This was enough to keep the sleepy fellow awake and reduce him to despair. In vain he snapped at his tormentor; the wary coon trotted to the edge of the enclosure, and scarcely had the badger composed himself before he was at his old tricks. One day he was too severe with the badger, which went off growling and rolled into his hole. After awhile he put his head out on account of the heat and went to sleep thus intrenched. The coon saw that he could not expect much attention from his friend under these circumstances, and was about to set off for home when the badger suddenly awoke and stretched his narrow red mouth wide open. This so surprised our hero that he turned back to examine the rows of white teeth from every point of view. The badger continued immovable in the same position, and this excited the coon's curiosity to the highest pitch. At last he ventured to reach out and tap the badger's nose with his paw. In vain, there was no change. This behavior of his comrade was inexplicable, his impatience increased every moment, he must solve the riddle at any cost. He wandered about for awhile, apparently undecided how best to pursue the investigation, but reaching a decision at last he thrust his pointed snout in the badger's open jaws. The rest is not difficult to imagine. The jaws closed, and the raccoon caught in the trap, squirmed and floundered like a captive rat. After a mighty scuffling and tugging he at length succeeded in tearing his bleeding snout from the cruel teeth of the badger and fled precipitately. The lesson lasted a long time, and after it whenever he went near the badger's kennel he involuntarily put his paw over his nose." The above minutely detailed description shows how dominant the element of curiosity is, and in addition there appears a tenacity of memory plus something almost reflective, which found its expression in a quite fully developed new method of defense, namely, that of placing the paw above the snout when near the enemy. Raccoons, when not too wild, push their paws into all the pockets of the keeper which are accessible, and lift all movable parts of their cage and try to find out what is in or under them. Nothing escapes their notice in the long run. They therefore lend themselves readily to training or experimentation. Details illustrative of this last will follow in a later section.

Vocalizations. An interpretation of the vocalizations of the raccoon is possible only in terms of accompanying circumstance. The cry of the young for the mother is often so nearly like that of the human infant that unless one were accustomed to hear it, it would easily be so mistaken. A low growl, like that of the bear in quality, warns off the intruder. This is also given when in possession of food and fearing its loss. When the coon springs forward to bite there is the accompanying snarl resembling in pitch and quality that of other members of the dog and bear family. A common call in the woods is in character a purr, which approaches very nearly in quality and carrying powers to the tremulous note of the tree frog. It lacks a little of the shrillness of the frog note, and rises in pitch when uttered by the young instead of the adult. When the mother is nursing her young, she gives forth a note which is almost a perfect imitation of the sound of a humming-bird's wings as it pauses before the flower. The two sounds are so nearly alike in character, that I was myself deceived many times before I was able to locate it in the cage rather than in the vines which grew in the rear. The mother's warning note in danger is almost inaudible. If one attempts to sound the letter *m* short and explosively with closed lips, at the same time allowing the air to be driven in short blasts through the nostrils, a very fair reproduction of the sound results. Although the note is very low, I have never seen a young one fail to recognize it even in the midst of the fiercest tussle of its play. A note quite frequently heard in captivity either while at play or, indeed when angered, can be very nearly reproduced by placing the lips in the position for whistling, and uttering the *oo* of boot followed by *f—oof*. This vocalization must also be given explosively. This and the note above require some practice to imitate perfectly. That the vocalizations of the mother possess great significance, although differing but slightly in quality, is shown by the experience with No. 3 when she was captured with her young. The mother was taken from the tree first and placed in a large box. When No. 5 was captured and pushed into the box with his mother he made a vicious attack upon her, but she simply gave out a low purring note and the young fellow immediately snuggled up to his mother and became quiet.

Fear. When the raccoon is afraid, it trembles, crouches into some corner, backs off, and frequently turns and runs. Sometimes the animal stands perfectly still with the head lowered and back hunched, looking at and following every movement in the direction of the cause of fright. The former attitude is shown in the animal at the left in Fi5. 7, Plate II, the latter position is shown in Fig. 4 of the same plate. Often a low vocalization or a diminutive grunt accompanies every movement. Sometimes a coon just in from the wild will try to cover its head and thus, ostrich like, render itself free from all danger. Fright is usually accompanied by a relaxation of the anal and urinary muscles, resulting in defecation and discharge of urine.

Greed. That the raccoon is greedy goes without saying. An animal whose bare existence usually depends on getting all that can be secured is so instinctively. A raccoon will gather in one place all the food he can secure and sitting down upon it endeavor to keep all others away until the food is completely devoured. The method of dislodgment is to back in, push the would-be glutton from his spoil, and then reaching under the body pull the food to the new possessor. Sometimes there is a direct attack and scuffle. Food is not the only bone of contention. One animal will try to keep the others from the water basin. This activity, however, may change to a species of play. If the food is being given to the whole pack in small pieces, some will take their portion and stuff it in their mouths and return again and again until the mouth is filled to its utmost capacity. Often they will even throw away for the time being what is given them in order to be in the scuffle for the last rations. These activities are simply indications of the adaptation to wild conditions where periods of want follow hard upon those of satiety.

Play. The plays of the raccoon might be classified as: 1. Simple activity; 2. Feeding and fighting plays.

Plays of simple activity are those in which the animal plays for the most part by itself, as when it takes a small stick or wisp of straw, and rubbing it in the direction of its longer axis between the fore paws, raises its paws above its head during the rubbing until finally it falls or rolls over backwards. Catching a dangling rope with the teeth, or seizing a flying trapeze with all four paws and swinging on it, or going through gymnastics on a horizontal bar, also belong in this class. One of the raccoons, after jumping from the roof of the inner cage to a flying trapeze, swung for a few minutes and then fell some three or four feet to the floor of the cage, landing on her back. As if angered by the fall and giving the trapeze the credit for the mishap, she climbed to the top of the inner cage again and attempted to wrench and gnaw it from its fastenings. Here would seem to be the beginning of an association of immediate cause and effect. One of the young coons used to sit at night in front of the swing door to one of the sleeping boxes and raise it up and let it fall for half an hour at a time. I was never able to discover any particular reason for this activity, though there may be an element of pleasure in the rhythmic sound produced. A horse will sometimes move a carriage against its cramped wheel in such a way as to produce a rhythmic sound apparently for that purpose. Sometimes a coon will sit on its haunches and beat a tattoo with its fore paws on the floor in front, with no other apparent purpose than its own amusement. Tipping over the water basin as soon as it was filled was indulged in by both old and young until I was compelled to put the water in an earthen jar too large and heavy to be tipped over. Catching flies from the walls of the sleeping boxes is a favorite amusement on sultry summer afternoons. Twice I noticed young ones in the act of sucking their paws after the manner of bears.

Among the plays which seem to be related to feeding and fighting, we find the raccoon dipping its paws into the water basin and then thrusting them while still wet into the face of a pursuer; also rubbing the dry and empty paws in the same fashion as when washing food in water, this is often resorted to as a form of amusement during the time between feeding periods. Sometimes one animal will take a piece of food and try to conceal it or run away with it, while others follow and try to secure it. This kind of play takes place when there is plenty of food and all have satisfied their appetite for the time being, since the food itself will often be left for some minutes before being eaten.

The fighting plays appear also in the tussle of the young with each other, old ones among themselves, or the mother with her young. If two animals begin a tussle, they usually stand off facing each other with head down, back humped, and paws extended in front. There are two or three preliminary advances and retreats and then comes the set-to, when, very much as in the "catch-as-catch-can" wrestling, they seize each other with the fore paws and twist and turn in the attempt to throw each other over backwards and thus expose the throat to attack. Once down there will be a playful use of teeth, accompanied by more or less vocalization, followed by a quick break-away and an immediate, or delayed renewal of the tussle. If the young ones begin such play with each other, it often happens that the old ones begin a similar match among themselves. This last may be an instance of imitation, and will receive consideration later. When one of the young ones attempted to hang from a low trapeze and have a good swing, another young one came along and, thrusting his nose into the gymnast's stomach pushed him to the floor and then took the place on the trapeze himself, throwing back his head and playfulty growling at the discomfited fellow who usually took this as a signal to retaliate in a similar attack.

I gave them a rubber ball one day. At first they tried to eat it, but when they discovered that it would roll the play began in earnest. One would back off with the ball between his paws while another would try to secure possession of it. Its attractiveness lasted but a short time, however, and after the first few days they never played with the ball again.

If the mother and young are approaching each other from opposite directions the mother will sometimes catch the hind foot of the young one just as she is passing and turn it over on its back with a quick flip, the young one then attempts to defend itself with its feet in air, or by curling up against the mother who tries to rub its stomach with her nose. I find this seizing by the hind foot while bearing off with the shoulders to be a very common method of fighting among mature coons. The raccoon also fights on its back, so we probably have here the beginnings of a proper training for future struggles where the life of the animal is at stake. Sometimes the young ones by themselves, but more often the mother would catch one by the tail, or hind foot, either with the paws or teeth, and drag it backwards across the cage. This usually resulted in a tussle, in which the victim on its back tried to seize the head and neck of the attacker with its fore paws and pull it down within reach of its jaws.

The study of Dr. Robinson (30) on ticklishness has evident bearing on this kind of play, and from him I quote:

> "Not only every part of our physical frame, but every instinct and appetite, either is, or has been at some past stage of human history, necessary to secure the survival or prosperity of the race. The baring of the teeth, the defensive movements of the limbs; the attempts to protect regions from attack as neck, armpits, and groin are characteristic of tickling pursued to a vigorous outcome. We are dealing with but one type of ticklishness, viz., that which is especially present in early life—when it is plainly associated with a natural desire or appetite which is intermittent, needing the subject to be in a receptive mood, and which is always associated with laughter and play. This form has a social significance, which the others, as irritability of the mucous membranes, on the palate and in the nostrils, or the palmer surfaces or soles of the feet, have not.
>
> "The more ticklish regions of the body both in man and animals are chiefly the armpits and contiguous parts; the ribs, especially where they join the abdomen; the front and sides of the neck, especially just above the collar-bone; the upper and inner parts of the thigh, over the region known to anatomists as "Scarpa's Triangle"; and on the limbs, the parts behind the knee and in front of the elbow.
>
> "Three points are plain: 1. All the young creatures which obviously take pleasure in being tickled—which have the appetite in a marked degree—are naturally playful, and appear to take a special delight in romps of a rough-and-tumble character, which are essentially mock battles. 2. The regions which are especially ticklish and most carefully defended in these games are those which, in a serious fight with formidable teeth and claws, would prove most vulnerable. 3. All these animals, with the exception of man, are armed in this way, and settle their differences by adroit use of such weapons.
>
> "Hence, a young dog or ape which, in the innumerable sham fights of its youth learns to defend the axillæ where a single bite might sever the axillary artery; the neck with the carotids and windpipe just under the surface; the flanks, and borders of the ribs, where a

comparatively slight tear lays open the abdominal cavity; and the groin where the great femoral vessels lie close to the skin, would, without doubt, be vastly better equipped for the fierce combats for supremacy in after life than an animal which had not undergone the same elaborate training. Warfare becomes more and more a matter of education, tactics, and strategy, and less a matter of brute force, as the scale of intelligence is ascended. Innumerable feints and methods of attack appear which are countered by a series of guards equally elaborate. Strategy depends upon experience, adroitness, and adaptiveness and not upon inherent instincts. It must be learned; and a young animal which had not the advantage of an education derived from sham fights in early youth would be as helpless, when brought face to face with an experienced foe, as one of us who knew nothing of fisticuffs or sword-play would be, if he were pitted against a practiced pugilist or fencer."

One of the most interesting observations upon the play of these animals was made during the first week after I secured the mother and her three young ones. When no one was looking on they used to sally forth from the little inside sleeping boxes into the yard of the large cage. The mother was always leading and each little fellow followed with one of his fore paws on the hip of the fellow just ahead. This seemed like a sort of follow-the-leader game.

In rare instances there appeared activities which might be interpreted as having sexual significance. Such would be those where one animal seized the other by the thighs with the fore paws and held it thus for sometime, or simply dragged it about the cage after having thus seized it.

Hibernation. Attention has already been called to the ability of the raccoon to adapt itself to variations of diet. A study of its life during the first and second winters seems to point to a similar adaptability to prevailing differences of temparature. The characteristics of the hibernating period, as slight beating of the heart, apparent cessation of respiration, loss of fat accumulated during the summer and autumn in supplying the constant demand in the production of heat energy, the necessity of seclusion lest the shock of sudden waking with its heavy demand upon circulation and respiration should cause immediate death; these are all familiar to the student of the habits of mammals undergoing the profound sleep of the hibernating period. The raccoon, however, does not sleep as profoundly as the bear. The constant loss of moisture, compels it to come out of winter quarters and seek the water. In the wild when they come out they sometimes travel for a mile or more, either returning to their own den or turning in with another coon, with which they pass the remainder of the period.

During the first winter my animals went into hibernation in an apparently normal way. They used to come out about once in two weeks for water. At such times they seemed to be getting weaker and weaker. Their actions were those of animals which had been in one position a long time, so that the trembling of the body, and tottering gait might have been due to a cramp from which they had but just found release. On coming out permanently in the spring, one of the animals was taken sick. She lost all her strength. If she attempted to climb or stand up on her hind legs, she would fall to the floor in a helpless condition. The enteric plug of the coon is not as large and hard, proportionately, as that of other hibernating animals, but this animal acted as if poisoned by the retention of the fæces and urine. Proceeding to treatment on the basis of this diagnosis she made a quick recovery after her relief from the disturbing cause.

During the first winter the cage was out of doors. During the second winter, since living in the city rendered it impracticable to keep the cage outside, it was placed in the basement of a stable. This was commodious and well lighted, and without artificial heat. The differences in temperature between inside and outside during the season were of but a few degrees, and yet none of the raccoons went into hibernation. Two others which came in, one about the middle of the winter and another almost at the end, went through the normal process of gradually waking. This failure to hibernate has thus far shown no determinate effect. In fact the physical condition of the animals seems higher since they have been able to avoid the usual depletion of the period. The lower latitudes of the geographical range of the raccoon offer conditions of such a character that hibernation is unnecessary, but the interesting feature of this observation was that so small differences of temperature seem to have been sufficient to cut out the period and habit altogether.

Disposal of excrement. Raccoons in the wild drop the fæces wherever they happen to be and leave them exposed. This is one of the means used by hunters for tracking them or determining their presence in a locality. It is also likely that from the observation of the fæces thus dropped, the Indians acquired sufficient knowledge of their feeding habits to attribute to them the conversations on the effects of certain wild fruits, which we find in the Indian folk-stories.

At first my animals in captivity followed the habit of the woods, exercising no selection of place for the deposit of the fæces. When confined in a small cage of two compartments, an upper and a lower, they soon made a choice, one of a corner in the lower compartment, the other of the entire upper compartment. When put into the larger cage, they gradually formed the habit of depositing behind the inner cage in a narrow but somewhat concealed passageway. When moved into a still larger cage with other animals, where the floor was covered with sawdust, all soon selected a definite part of the floor and began covering the excrement. The seven animals which are now together in a still larger place of confinement, have two places within the area, and at one of the other of these they all, with but rare exception deposit, urinate, and cover.

Something similar, so far at least, as regards the constant use of a particular place is not infrequently to be observed in the case of other animals. I am not aware, however, that the taking on of the covering habit had been noticed. In any case the observation is a most interesting one. We have a stereotyped form of behavior which, if observed in its final steps alone, would unhesitatingly be pronounced instinctive, actually acquired in a comparatively short time and under observation. That it is a response to some special feature or features of the life in confinement can hardly be doubted, and the question is natural as to how far other stereotyped and seemingly instinctive forms of activity in these and other animals are likewise plastic and owe their seeming fixity to the fixity of the conditions in which the animals usually pass their lives. We may even ask the further question whether man's greater plasticity is not in large measure the reflexion of his complex and varying environment rather than a special endowment.

These questions are of course more difficult to answer than to ask and it is not our purpose here to try to answer them. Even the attempt to fix upon the special features of the life in captivity that are responsible for the change must be largely conjectural. The uniformity of place selected may have an element of imitation in it, to be discussed later, or may be the result of an association by which the odor of the excrement tends, when the time of excretion is near, to suggest the act. The disagreeable association of odure with no possibility of escape for an animal not accustomed to its presence may account for the covering which conceals what cannot be left behind and thus out of sight.

Temperament. Whoever has observed animals, either wild or tame, for any considerable time, sooner or later discovers that they, like the human animal, have widely differing temperaments, and manifest varying characteristics, which may depend in part on physical organization but find proximate cause in changes in weather conditions, food, or other disturbing factors not so readily classified. While it may be less possible even than with man to select distinctive physical marks of temperament, such as the color of the eyes, hair, and skin, temperature, character of the pulse beat and quality of the flesh, yet impetuousness of action, sluggishness, cruelty, ferocity, solitary or gregarious tendencies, moroseness, stupidity, non-irritability, and such like, when observed, go far towards giving a more perfect understanding of animal life. Indeed, these temperamental differences are often as well defined as in the case of human individuals. Some animals are of the nervous, others of the phlegmatic, and still others of the bilious type, if we may be allowed to designate them thus for purposes of description. Those of the most purely nervous type are much more active, make many more movements in experimental work, and yet accomplish the task in the same or less time than their more slowly moving, accurate and phlegmatic fellows.

A blow with a stick may mean to one animal a never-to-be-forgotten injury, while to another it merely serves as a corrective. Raccoon No. 3, whose photograph appears in Figs. 4 and 11, and at the right of Fig. 7 in Plate II, has never ceased to remember a blow which was given in a thoughtless moment, when the master lost his temper. She is always suspicious, and makes use of every opportunity to spring at any person, for all alike are enemies. She will, however, stand up on a box at the opposite side of the room, or even in the middle of the floor and beg for food as if she were the most affectionate of all. (Fig. 11, Pl. II.) Go near and you are met with a growl, a spring and sharp teeth. (Fig. 7, Pl. II at the right.)

Figs. 3, 6, and 10, Pl. II, represent an animal at almost the opposite extreme. She has been punished for wrong doing but never resents it. She will come to you, sit on your knee, climb to your shoulder for food, and if she thrusts her pointed nose into your face, she never offers to bite, snarl, or intentionally scratch you. Fig. 1, is much of this same type, but being blind he must be touched gently, as he can only thus distinguish friend from foe. Fig. 2 shows an animal which was at the first timid and somewhat morose. For more than a year it was impossible to either coax or whip him into obedience; then suddenly, without any apparent cause, he changed entirely. He began to climb into the lap, to eat from the hand, and to do as was desired. He seems still to hold unpleasant memories of a stick which his master seldom carries now, and the passing of the stick may account for the absorption of the field by new and more pleasant associations.

A change of scene seems to affect all alike, save one. It usually takes about three weeks for them to settle comfortably into new quarters, and go about their experimental tasks. When they were moved into the university building I was not prepared to find them refusing to work even at their most familiar tests for more than ten weeks.[1] They even refused to take their food from the hand, save one, as noted above. This is an old coon. Fresh changes may arise but she is never disturbed. She goes about as if all the new were old. I have never been able to get her full history, and so suspect that she may have been in captivity before, and possibly have escaped.

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[1] A series of extraneous disturbances to which they were subjected during the earlier part of their stay in the university building had much to do with their long period of nervousness.

The field of observation in temperamental differences is a large one, but it is fertile with clues to all kinds of experimental problems, and its possible effects have, perhaps, been too little regarded by experimenters generally, especially when experiments have been made upon but few individuals.

EXPERIMENTS.

Animals experimented upon. It is hardly possible to give more than an approximate estimation of the age of the animals which I have had and now have, in view of the fact that they were all secured from the wild and so fully grown as to put accurate figures out of the question. The figures are simply estimates based upon close observation by myself and others acquainted with the life of the raccoon in the wild. They are rough approximations, but are believed to be not far from the truth.

No.	Sex.	Approx. age.	Remarks.
1.	Female.	4½ yrs.	Trapped.
2.	Female.	4½ yrs.	Trapped,
3.	Female,	3 yrs.	Taken from tree with young.
4.	Male.	1½ yrs.	Young of No. 3.
5.	Male.	1½ yrs.	Young of No. 3. Died Feb., 1907.
6.	Male.	1½ yrs.	Young of No. 3.
7.	Female.	Age unknown, old.	Bought. Perhaps previously in captivity.
8.	Male.	2 yrs.	Obtained from Penn. Escaped, recap.
9.	Male.	2 yrs.	Obtained from Penn. Escaped.
10.	Male.	2 yrs.	Obtained from Penn. Escaped.
11.	Male.	3 yrs.	Trapped, escaped, recaptured, died.
12.	Female.	2½ yrs.	Trapped, died.

The ages given are either for the summer of 1907, or the age at death or escape. I have had to do with about equal numbers of each sex, and have not been able to determine that sex makes much difference in the matter of intelligence; it is rather the individual temperament and character which count.

Housing. During the three years which have thus far been given to a continuous observation of these animals, their quarters have been changed and enlarged as numbers increased and experimental conditions made greater demands for space. In the beginning, the first two animals were kept in an enclosure of chicken wire fastened to a wooden framework 6 ft. x 4 ft.

x 6 ft. The wire was reenforced by interwoven iron wire of No. 18 gauge. Inside this enclosure was a small two-storied closed cage in which the animals passed the nights and also hibernated during the first winter. In the spring the third animal which was trapped, No. 11 in the above list, escaped from this cage and thus made necessary the construction of larger and more secure housing. A cage 7 ft. square was constructed, with a wooden framework, which supported heavy iron screens of 1½ inch diagonal mesh, such as is commonly used in banking houses. The rear of this cage was shut in by a series of four two-storied sleeping boxes, so arranged as to be occupied singly or all thrown into communication. This offered facilities for the isolation of any particular animal during experimental periods, and at the same time made it possible to gratify the animals' propensity to crawl in and out through dark and light places. The third move was into an enclosure inside the University buildings. This is 14 ft. square, constructed on two sides of sheathing to a height of 7 ft., the remaining space above being shut in by heavy poultry wire of ½ inch mesh. The inside walls of the building form the other two sides of the enclosure. Two large windows look out on the street, which, however, is some distance away. The wire and the boxes and stands placed on the floor provide opportunity for exercise in climbing about.

Feeding. The sorts of food upon which the raccoon will thrive have already been mentioned (p. 450). In the matter of feeding three meals a day and regularity has been the rule. Bostock's (4) six days' feeding and one day's fasting has also been adopted. This equalizes somewhat the great differences between the conditions of food getting in the wild and in captivity. In so far as possible the times of experimentation have coincided with the feeding periods. In experiments like those in color discrimination, where variation in natural illumination was necessary, there was a departure from this rule. The ration has been fairly uniform. Neither a condition of satiety nor hunger has been sought, but such as seemed on close observation to be that of highest efficiency. It is somewhat significant that, after a period of work, the coons usually retired to the sleeping boxes and took a nap of greater or less duration. Whether this was an alternation of sleeping and eating or one of work and sleep is not clear. Probably all three factors entered in some degree.

Apparatus. In the experiments to be described, the food was placed inside a wooden box 15″ x 11″ x 12″, in the front of which was a door 6″ x 5″, which the animal was required to open in order to secure the food. (See Plate II, Fig. 5.) The door was adjusted to open outward when it was once relea by the locking device. The simple devices used were:

1. *A button.* This was so placed and adjusted that it must thrust downward through an angle of about 40°. The button placed at the upper right hand corner of the door during one se of trials, and at the upper left hand corner during a second series.

2. *A vertical gate hook.* This was placed at the middle of door. It was adjusted to move to the right in one series of trials, to the left in another series.

3. *A bolt.* This was of the common slide-bolt pattern. It placed at the left of the door, and adjusted to slide to the left.

5. *A T-latch.* This was placed vertically at the middle of the d and adjusted to move to the right in one series of trials, and to the in another series.

6. *A lift-latch.* This was placed at the right of the door, and justed to be moved through an angle of 90° in order to release door.

7. *A plug.* This was placed on the top of the box, and so c nected with a bolt inside the box that pulling the plug drew up bolt and so released the door.

8. *A horizontal hook.* This was placed at the left of the door had to be lifted in order to release the door. It was the same h that had been used in the vertical position.

9. *A bear-down lever.* This was placed at the right end of box, and adjusted to fasten the door on the inside. Lifting the in end about an inch released the door.

10. *A push bar.* This was placed at the right end of the box. was adjusted to fasten the door on the inside. A thrust of ab three-quarters of an inch released the door.

Beside these simple locking devices, which were used sing the door for certain experiments was held by two or three such simple fastenings. These will be referred to as *Grou* The elements of the groups were familiar to the animals fr their use singly, and could, of course, be operated in a order. The groups were constituted as follows:

11. *Group I consisted of two buttons*, one at the upper right ha corner of the door and one at the upper left hand corner.

12. *Group II was the same as Group I with the vertical gate h added* at the middle of the door between the buttons.

The *Combinations*, like the groups, were built up from t simple locking devices already familiar, but they differed fr the groups in requiring that the elements should be operate if at all, in a definite order.

13. *Combination I consisted of four locks*, a bear-down lever at t right end; a plug at the top; a button at the left of the door; and vertical hook placed a little to the right of the middle of the do The entire mechanism was so constructed that each simple loc while a part of the combination, yet fastened the door by itself. T outside and inside views of this combination are given in Figs. 5 a 8, Plate II. When the lever numbered 1 was properly worked it fa tened itself so that it could not be moved back again and refasten t door, and at the same time made it possible to pull up the plug When the plug had been pulled, the button 3 could be turned; a

when the button had been turned, the hook 4 could be released, and the door would fly open.

14. *Combination II consisted also of four elements:* a push-bar at the left end; a bolt at the left of the door, to be thrust to the left; a button at the right of the door, to be thrust downward; and a kind of hook, known at the hardware dealers as an inside-blind catch, placed at the top of the door, to be thrust to the right. The internal character of the mechanism was similar to that of Combination I. In this combination, as in the other, the elements were so arranged as to prevent the accidental relocking of any one of them after it had been operated in its proper turn.

All the simple locking devices were set to move at between 300 grms. and 400 grms. It was not possible to adjust them accurately in the case of the combinations, on account of the combined lever system and the crudeness of the construction. In the combinations, therefore, there was much variation in the amount of force necessary to move the fastenings.

Method of Work. Food was placed inside the box, the door closed and fastened by whatever device was in use at the time. The closed box was then placed in the cage and the animal turned loose to investigate. No other incentive was given than curiosity and moderate appetite, possibly intensified by the smell of the food concealed in the box. At the first the box was not fastened to the floor, but it soon became necessary to fasten it, because of the rough handling it received unless so secured.

The times required for the working of the fastenings were taken with a stop-watch reading to fifths of seconds, but the curves presently to be considered were all plotted from the nearest readings in seconds. The time taken, included the entire period during which the animal was in contact with the locking device. If his attention was distracted note was taken of the same, and such and all known sources of error eliminated from the learning curves.

Each animal was given forty trials on each locking device. These forty trials were not always consecutive, because the animal often left the box after ten or twenty trials. Sometimes this was due to a satiety of food; at others it was due to lack of interest. A period of several hours or even an entire day might thus intervene at times between two consecutive trials.

The successful operation of the mechanism in one second was taken as the standard for perfection in the case of the simple locks. In the case of the combinations, the animal was considered up to standard when it worked the apparatus ten times in succession without error in the order or failure in muscular adjustment to the different locking devices. The times of such standard performances was uniformly about five seconds.

RESULTS OF EXPERIMENTS.

Variations in the attack on locks. The tests indicate tha each animal shows some individuality in the method of openin the locks, though different methods are employed at differen times. Sometimes the variation appears in the way in which th animal approaches the box, and this often involved a characteristic method of attack; again in the order of working a group and often in bringing into action not only the fore feet, bu hind feet, nose and even the weight of the body. Age als makes a difference. Old coons work with reserve, directness and accuracy; while the young ones, if they work at all, mak a much larger but less regulated output of their energy. A fe illustrations will make this matter clear.

When the button at the upper right hand corner of the doo was first given to No. 1, she used to reach down the front of th box while standing on the top and thrust the button with the le paw. After a few trials she changed. Going around back c the box to the right end and reaching around the corner, sh pulled the button towards her with the left paw. Sometime she would come to the front of the box and thrust the butto with the right paw, or with the teeth. Nos. 2, 3, 4 and 5 always went to the front of the box. No. 2 used her nose. No 3 used her right paw and standing at the left thrust the butto from her. No. 4 and No. 5 used both paws, and often teet and paws.

In the case of the bolt at the left of the door, No. 1 has always worked it from the top of the box, by reaching down th front and thrusting the bolt to the left with the right paw This method may have been borrowed from the position firs assumed in the case of the button. No. 2 stands in front an a little to the right of the door and thrusts the bolt to the le with the left paw. No 3 stands at the left end of the box an pulls the bolt towards her with the right paw. Nos. 4 and adopt at different times the method of No. 2 or that of No. 3 They have never worked from the top of the box thus far.

All five with practical uniformity now repeat the position an method finally fixed upon, whenever the same device is presented, even after intervening periods of days, weeks, or months When Group II was given to No. 1, she used at the first t open the button at the left by standing at the left end of the bo and pulling the button towards her with the right paw ; the she would walk round behind the box to the right end an reaching the hook with her left paw pull both hook and butto towards her. One variation of this method was to stand i front of the door and thrust the button on the left with the lef paw, and the hook and button on the right with the nose. An other variation was, standing in front and placing the right paw on the right button, the left paw on the left button, and the nose on the hook, to open all at once. She had made use of the two paw method in the case of Group I, so that it was but a slight addition to use the nose on the hook. The reason for the use of the nose on the hook came from an earlier experience with it when it was the only lock in use. In attempting to open the door on the twenty-fourth trial of the vertical hook series, this animal caught one of her toes between the door and the hook. From that time she adopted the nose method and after more than a year, with no intervening trial she still persisted in it. This seems like in character to the action of the coon with the badger as related by Groos, and both show, in contrast to the repetition necessary to perfect the procedure in such things as opening the locks, that certain elements of behavior, probably those that are supported by strong instinctive tendencies, may be determined once for all by a single experience. The same thing appears also in No. 3's continued resentment of the blow already referred to. (p. 461.)

One of the most interesting and illuminating observations which I have been able to make as to the method by which complicated procedures are perfected is the following: One of the young coons, No. 5, was at work on the beardown lever at the right end of the box. It had just been preceded by the bolt at the left of the door. Although the bolt had been removed, leaving the front of the box smooth, No. 5 still devoted his attention to the place where the bolt had previously been, and nearly wore himself out trying to push something from the place where nothing was present. The previous association was too strong to be easily replaced. In the midst of his endeavors, during which he literally rolled over and over in front of the box, he assumed the position of standing on his head. While working in this position his right hind foot slipped off the top right hand corner of the box, and striking the lever pressed it down and opened the box. From the fourth to the eighth trials he would assume the up-side-down position, and work at the door until his foot slipped off the corner of the box, depressed the lever and so opened the door. His attention, however, was never shifted from the front of the box. Finally, in the midst of his gymnastics, his foot slipped off as usual, but did not strike the bar heavily enough to depress it and open the door, although it remained in contact with it. Distracted by this, he turned and using his fore paws thrust the lever down and opened the door. The next day he was working the lock by slipping his foot from the top of the box to the bar, and then turning about and using his fore paws. On the twenty-eigth trial of the series, he went directly to the bar and opened the door by thrusting it down, but he still sat down on his haunches, placed his hind foot on the lever, and then assisted with the fore paws. It is fair to assume that in course of time something more easy than this awkward performance would have taken its place,—probably in the end the depressing of the lever with both fore paws, or even the right one alone; but the final steps in the short circuiting were cut off by No. 5's untimely death.

The above seems like a case of a stimulus having to beat out its path over an unblazed trail in a nerve wilderness. It is typical, however, of much of the learning of animals and indeed in a measure of all learning where the way is not opened up by hereditary predisposition or expert instruction. Repetition of imperfectly adapted movements brings about a final happy accident. Many rehearsals of the activities leading up to this follow. Continued practice, in ways not altogether clear as yet, leads to the gradual elimination of the extra movements; the unessential parts of the process are sloughed away and the essential solidified. The steps by which perfection is reached are very short and blindly taken, the new habit arising by slight successive modifications of one already in existence. Perception of the essential relations, if present at all, is dull and stupid to the last degree.

Restated from the beginning, it would seem that the smell of the food, intensified by the complex mental activity of curiosity, in some manner lets loose a surplus discharge of motor activity. This sooner or later leads by accident to an agreeable result and a consequent emphasis upon the nervous tract then in action. In this particular case that underlying the association between the position with the hind foot on the lever and the opening of the door. When this position alone happens to be insufficient the fore paws are brought to assist as the readiest means for that purpose—the members most responsive to any need of that character. So far our raccoon had gone.[1]

Burk (8) enumerates the "chief accessory lines of development that distinguish human movements as such, as: "(1) the breaking up of old bilateral and simultaneous tendencies, characteristic of central movements; (2) the growth of independent movements of smaller parts that previously only moved in conjunction with larger wholes; (3) the co-ordination of various series to form long and complex sequences as we finally find them illustrated by writing, sewing, piano-playing, etc.; (4) the development of precision and accuracy; and finally (5) the response of different movements to a great variety of different stimuli." These all seem to find a ready illustration in the development of animal 'activity as well as in the child, although in the former case they may be placed on a somewhat lower level.

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[1] On this general matter see the paper of Dr. Meltzer, No. 22 of the bibliography at the end of this paper.

No. 1's experience with Combinations I and II, furnishes material of similar significance. As before stated, the locks of the combinations were not uniformly adjusted as to the force necessary to operate them (p. 465). Reference to Table I, shows the two chief sources of error, and the average number of errors per trial, in groups of ten. Thus 4.6 is the average number of errors in order, made in the first ten trials on Combination I, and 8.5 is the average number of errors of force applied in the first ten trials on Combination I. Lines 3 and 4 give similar results for Combination I, after a period of 286 days, with no intervening trial. Lines 5 and 6 give results for Combination II, as lines 1 and 2 give them for Combination I. The table seems to show that the memory of the order is more readily perfected, than that of the muscular adjustment required for each particular locking device. It would seem as though the order was something that could become definitely fixed while the muscular adjustment could proceed only to the point of pulling till something gave way. The reduction of errors in order is, perhaps, more uniform and less erratic, than that of errors of applied force.

TABLE I.

Relative proportions of errors in order and errors in force in working the combination locks.

Combination 1.	1	Errors in order.	4.6	8.0	9.3	8.4	7.6	8.4	1.1	2.1	1.8	0.7	0.0	0.0	0.0
	2	Errors in appl'd force.	8.5	10.4	11.1	8.8	10.4	10.3	5.6	5.9	3.4	0.9	0.0	0.0	0.0
Combination 1 after 286 ds.	3	Errors in order.	3.5	0.7	1.4	0.4	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
	4	Errors in appl'd force.	10.5	0.7	3.4	0.4	0.9	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Combination 2.	5	Errors in order.	18.6	7.2	2.8	5.5	4.5	4.5	3.0	2.8	0.5	0.0	0.0	0.2	0.0
	6	Errors in appl'd force.	24.1	24.2	7.3	7.3	4.6	5.0	3.0	2.8	1.7	0.0	0.4	0.5	0.0

It is highly significant that the order was a constant quantity throughout the entire series of trials with each combination, while the force necessary to move any individual part of the combination was never quite the same. Proper co-ordination and development of precision and accuracy finally appear, however, so that once the proper conditions are given, for the starting of a series of movements, they then follow with due force as well as in proper sequence.

In this connection No. 1's method of working the plug is also interesting. When the plug was first given, the method was to stand on the top of the box and pull out the plug with the teeth. The paws were often used to assist while in this position. After the first ten trials, she discovered that she could stand in front of the box and reach up and pull the plug out with her teeth. A few more trials changed the method to standing in front of the box and reaching up behind the plug and jerking it out with the left paw. After this final method became settled, the animal used to come to the front of the box sit up and place her paw behind the plug, look around in a seemingly careless fashion and jerk the plug out without giving any special attention to the process. It was almost as if she would think the device too easy to need anything more than a passing notice. Some such explanation may be plausible for different characters present in the learning curves, yet to be considered.

Cole (10) found with his raccoons, contrary to Thorndike's experience with the cats, that the animals never began work at parts of the box where fastenings had previously been, but went at once to the place of the fastening then in use. The raccoons "did not paw at the place where the loop had been nor did they claw at the loop or button when the door was open. I tried moving the loop from place to place in the boxes. Not once even did they claw where it had been; instead they attacked it at the new place with one direct movement." The trials of my coon No. 5 with the bear-down lever, during which he worked with so much energy at the place where the bolt had been, as described above, point in exactly the opposite way and lead me to think that Cole's results must be exceptional.

My No. 3, an old coon, the mother of No. 5, also behaved in a way to lead to a similar conclusion. After she had pushed back the vertical hook and the door had sprung open, she used to sit in front of the box, pull on the hook and then look inside to see if there was any more food. It was rather humorous to see her repeat this performance again and again. Later she dropped it and has never shown a like tendency since. Whether she gave evidence here of a perception of simple relations, as "this hook gives food," which later changed to "this hook and hole do not give food," may be open to question. When, however, the door was fastened and food placed inside, she never refused to work this lock. The above does not seem to differ much from the habits of the wild. The animal certainly senses certain conditions as indicative of food, and certain other as indicative of no food. The resulting associations persist and strengthen themselves in the first case. The latter cease to stimulate.

Old coons soon discover the difference in pulling at the long or the short arm of a lever, as in the button, while the young coon will work indifferently on the long or the short arm just as it happens to attack the device. It seems, sometimes, as if there were a kind of intelligent husbanding of energy, which shows itself with age. The tremendous output of energy in youth may be necessary to proper growth and the acquisition of experience, and possibly its numerous errors are thus intimately connected with the learning process.

In the working of Combination I, Raccoon No. 1 reached the arbitrary standard of ten perfect performances in trials ninety-eight to one hundred and seven. In the case of Combination II, the standard was reached in trials one hundred and twenty-five to one hundred and thirty-four. Since the first learning of the order, some variation has occurred in the animal's working of the locks in Combination I, all, however, in the direction of economy. The bear-down lever is now often opened by stepping on it with the hind foot as the animal mounts to the top of the box for the plug, and the button and hook on the front are usually opened by one movement of the right paw. This activity is similar in character to that of No. 5 already described on p. 467. No. 1 is shown in the act of doing this very thing in Fig. 9, Plate II. The tendency to "short circuit" is clear again in this instance.

In the tests with the combination locks an opportunity occurred for noticing the persistence of certain impressions and the signal importance of making no false steps in training. In the first fifteen trials on Combination I, the number of errors was comparatively few. During the next three trials, it was discovered, that while the combination was a perfect working mechanism if worked forward, an accidental reversal of the first element, the bear-down lever, would lock the box again. The animal did this very thing once, and after the device had been so arranged as to make its repetition impossible, more than fifty trials were necessary to bring the errors down again to the level of the first fifteen trials. "Fogging" an animal in this manner while under training will obviously play havoc with figures and render the results difficult of interpretation. Combination II had as one of its elements what is known at the hardware dealers as an inside-blind catch, with which the raccoon had had no previous experience. It proved, however, no more difficult than other parts of the combination. The animal seems to have attained to a sort of generalized behavior with regard to fastenings which made small differences of little consequence.

After the two combinations had been learned perfectly, the raccoon would work them indifferently. If she were given one or two trials on Combination I and then it was replaced by Combination II, the animal never made the mistake of beginning on either apparatus as if it were the other. In view of the fact that the two combination boxes were not exactly of the same dimensions, and differed somewhat in color, there is little doubt that this difference sufficed to touch off in each case through the association centres the proper order and relation of movements for the particular combination presented.

The animal tends strongly to approach the boxes from one direction only. If this avenue of approach is blocked up, a new adjustment to the new condition will, of course, take place, but the old fastening from a new side presents about the same difficulty as a new fastening.

Time Required For Working the Several Devices.

The figures in Table II are typical of results obtained by this method of work with Raccoon No. 1. The figures are the times in seconds for the forty trials on each of thirteen simple locking devices and the two Groups. Similar tables for the other animals have been prepared but as they would present no significant variations other than those that will be mentioned as the exposition proceeds, it has been thought best not to publish them. The locks in the case of this animal were learned in the order here given.

The large figure 1's mark the record of the trial in which the animal worked the lock in one second for the first time. The medium figure 1's mark the record of the first trial in which the standard of one second was permanently reached.

The Learning Curves. In order to give a graphic representation of the nature of the learning process, as shown in the animal's experience with the locks, the following method was adopted, for which the writer is indebted to the kindly assistance of Prof. W. E. Story, of the Mathematical Department of Clark University. First, all trials affected by known sources of error were eliminated from each series. Then, since the working of the lock in one second had been determined upon as the standard of perfection, the number of trials necessary to attain this standard permanently was determined. Each series naturally gave a different number. Reference to Table II, for instance, shows that in the series for the bolt the standard was reached on the seventeenth trial, while in the case of the horizontal hook, the standard was reached on the fourth trial. The times of the trials were then plotted as

TABLE II.

Time required for working the several devices in the case of Raccoon No. 1.

Trials.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
Butt. R.	180	81	316	144	179	18	16	32	8	6	14	6	11	74	2	3	3	2	4	4	60	30	6	7	3	2	**1**	1	65	15	**1**	1	1	1	1	1	1	1	1	1
Butt. L.	24	46	40	9	2	35	14	16	6	34	2	21	6	4	3	2	9	**1**	4	9	4	6	10	4	6	11	9	6	1	5	3	2	4	6	2	**1**	1	1	1	1
V.Hook.	47	90	10	32	4	2	**1**	3	4	4	4	40	2	1	4	**1**	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Bolt.	190	60	10	**1**	4	5	10	1	3	4	8	5	2	1	1	2	**1**	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
T-Latch L	25	3	**1**	5	10	12	2	3	5	14	15	5	5	2	1	1	4	1	1	1	1	1	2	1	13	1	1	1	12	1	6	1	1	4	**1**	1	1	1	1	1
T-Latch R	35	15	11	50	6	5	**1**	4	1	3	3	1	2	**1**	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Ct-Latch.	**1**	1	12	8	2	1	7	1	1	1	1	1	1	1	1	1	5	7	**1**	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
H.Hook.	5	**1**	5	**1**	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Lever.	72	30	2	**1**	1	1	1	1	1	1	1	1	1	2	1	1	1	1	1	1	1	2	1	2	1	2	**1**	1	1	1	1	1	1	1	1	1	1	1	1	1
Plug.	30	7	3	3	2	**1**	2	**1**	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
P'sh.Bar.	264	234	40	15	6	17	**1**	11	1	1	9	2	3	1	3	3	3	3	1	1	2	1	2	1	1	2	1	1	2	**1**	1	1	1	1	1	1	1	1	1	1
Grp. I.	16	9	9	80	6	14	9	39	8	5	4	3	3	3	20	12	22	8	6	3	2	13	10	**1**	2	3	2	2	**1**	1	1	1	1	1	1	1	1	1	1	1
Grp. II.	20	**1**	2	5	6	2	2	3	2	2	4	9	3	15	1	2	2	2	1	1	1	1	9	1	6	1	1	2	**1**	1	1	1	1	1	1	1	1	1	1	1

ordinates, and the number of trials from the first experience with the lock till the standard was reached as abscissas, as many equidistant points being taken along the horizontal axis as there were trials necessary to reach the standard. Thus in the case of the bolt mentioned above, the whole base line would be divided into sixteen equal intervals, giving with the point of origin seventeen points, while in the case of the horizontal hook, it would be divided into three equal intervals, giving four points. Proceeding in this manner, it was possible to superimpose graphically the curves for all the trials of an animal on all the locks. The average of all the times of the superimposed curves, for any single ordinate, therefore, determined a point in a generalized curve of learning which, as a whole, represents each animal's experience with all the locks. Several of the curves thus obtained are given in Plate I. Figs. 1-5 are the curves respectively of raccoons No. 3, 5, 4, 1, and 2. No explanation has yet been found for the elevations in curve No. 4 at intervals three and six.

The curves, with perhaps the exception of the first, have the usual form of the learning curve. They agree in showing at first a more or less rapid falling off in the time required to work the different devices, followed by a gradually decreasing rate of progress as perfection is neared. The curves, however, fall into three groups of somewhat different types represented by Figs. 1 and 5; 2 and 3; and 4 respectively. Comparing Figs. 2 and 3, with Figs. 1, 4, and 5, it appears that the young animal learns more rapidly at first, having perhaps more to learn than the old, as shown by the sudden and long fall in their curves, Figs. 2 and 3, while the old animal profits more evidently by experience, as shown by the uniformly low level of the curves in Figs, 1, 4 and 5 after about the eighteenth or twentieth interval. The continued unevenness of the curves for the young animals is undoubtedly due to the fact that their learning is accompanied by fumbling and distraction of attention for a long time. The variation between Figs. 2 and 3 may be due to the fact that Fig. 3 represents the trials of an animal that is partially or totally blind, and thus is compelled to rely more definitely on touch and smell, and so loses the directness attendant on sight. Figs. 1 and 5, with their peculiar rise at about the twelfth interval, do not readily lend themselves to interpretation. This is especially true in view of Fig. 7 which represents the combined trials of the above four animals on one locking device. This device was not the first one given, but one chosen after each animal had had some experience with locks. This particular curve represents the tests on the vertical gate hook. It is typical, I think, when all things are taken into consideration. It presents, however, the

PLATE I.

Fig. 1.

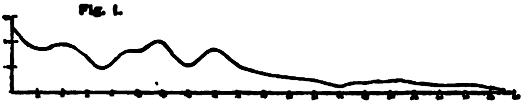


Fig. 2.

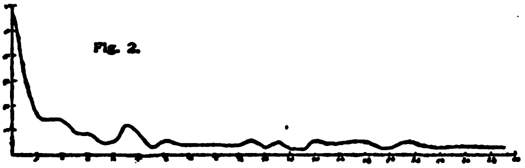


Fig. 3.

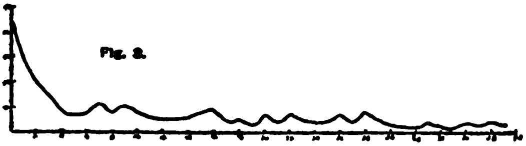


Fig. 4.

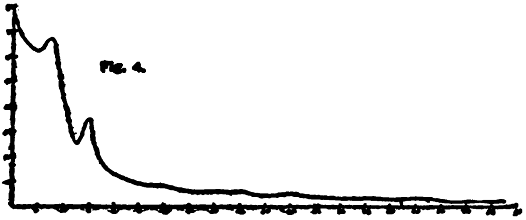


Fig. 5.

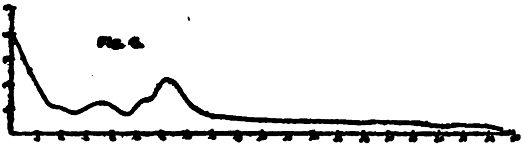


Fig. 6.

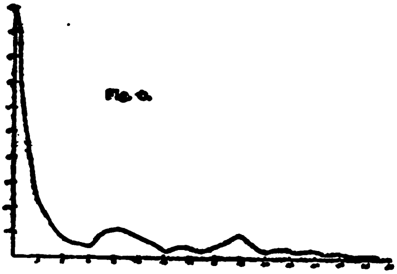
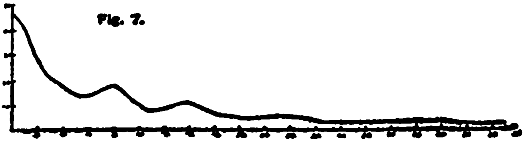


Fig. 7.



above mentioned peculiarities of Figs. 1 and 5, but has the same general character as all the curves. These rises may mean that after the first period of learning there is a short period of forgetting, or of inattention and indifferent effort. Familiarity with a given locking device would in such a case enter as a factor to increase the time. I have already described something of this sort in the trials of No. 1 on the plug (p. 470). We may have here perhaps the analogue of the "plateaus" which have received attention in the learning curves of human subjects. A comparison of Figs. 1, 5, and 4 would seem to indicate that there may be a temperamental factor in these elevations. The animals whose trials are represented by Figs. 1 and 5 are slower in all their activities than the animal whose trials are represented by Fig. 4.

Fig. 6 is interesting by way of comparison. This represents the series of thirty trials each on nine simple locking devices used by Kinnaman in his experiments with the monkeys, which were similar to those used by me with the raccoons under very nearly the same conditions. The figures are taken from his table and plotted on the same plan as the curves already considered for the raccoons. The general character of the curves is evidently the same for both animals. The value of the first experience seems, however, a little more evident in the case of the monkeys as shown by the steep descent of the curve. Indeed, the monkeys would seem to be a little less clever at the start. Elevations like those in Figs. 1 and 5 are to be noticed, but farther apart, falling on intervals eight and eighteen. The irregularity of the curve would suggest that the raccoon is less open to distraction than the monkey; and this might be considered by some as evidence of a lower level of intelligence on the ground that the coon works mechanically, but the opposite position might just as well be taken on the ground that the curve shows greater power of concentrated attention which is evidence of higher intelligence. My own position is one of uncertainty, since I have never had an opportunity to make general observations of the monkey's activities. These differences may also be of a temperamental character. It would seem evident on the face of the curves alone that the two animals stand fairly close together in the matter of learning to undo locking devices.

Combinations I and II were given to boys and girls to learn and the results of the trials were plotted on the same plan as the curves given above. Although the number of trials necessary to attain the standard fell inside of thirty, yet the character of the curve is the same as in the case of the animal.

The curves and the observations which they rest upon seem to justify the following general conclusions:

1. The learning curve for the raccoon follows closely the type of those found for other higher animals and for man.
2. The variations of method which appear in the animal's working of the locks and lead up finally to a permanent method seem to be simply the tendency to the formation of a "short circuit" by the elimination of useless movements, the permanent method being the shortest circuit for the raccoon.
3. There appears to be an increasing husbanding of energy, and profit by experience with increasing age.
4. There is an evident ability to respond to small differences in complex relations. How far the perception of such relations really enters is, however, for the present in doubt.

Experiments upon Association and Memory.

No one who has had to do with animals doubts that they remember, but the question as to the nature of the memorial processes is quite another matter. We have good reason to believe that the life of the animal lies very largely in the field of sense experience and an apparent recognition of relations may in any case prove to be nothing more than a reinstatement of the conditions under which the first association was formed.

During the tests with the raccoons on the simple locks, varying intervals of time broke the series of forty trials for each into groups of a less number. This was necessitated by the failing interest of the animal, but was readily accepted by the experimenter from a desire to discover if there was any well defined recollection from one period to another of the work done. It was soon evident, as had been expected, that the animal carried over from period to period some sort of an association, which resulted in a reduction of the time consumed in the working of the lock. It was then decided to give a crucial test to coon No. 1 using Combination I as the apparatus. The animal had been given preliminary trials with Combination II for a period of nine days, the intervals varying from seventeen to thirty-seven hours.

The results of these trials showed that memory is perfect for such short periods. The tests with Combination I extended over a period of 332 days, and the intervals varied from nine hours to 286 days, the latter period including the time of hibernation. The severity of the test is evident. Where the interval was short the memory was perfect. At the end of the long period there was a repetition of a series of trials similar in character to those made when the box was presented for the first time more than a year before. Only 24 trials were necessary to attain the perfect standard which had previously required 107 trials, and the times of these 24 trials were much shorter than the first 24 trials of the 107 trial series. It is thus clear that while complex associations lapse somewhat with long interval in which the original conditions are not preser there is yet retained a pretty fair knowledge of what was first presented. In the case of simple locking devices, howeve after an interval of more than a year, with no intervenin trial, different animals attack the lock with about the sam accuracy, and with no greater variation in the time consume than would appear in successive trials at the end of an origin series in which the time had been reduced to its lowest facto Simple associations would seem then to be quickly and perm nently fixed.

It is extremely easy to underestimate the difference in ass ciative responses that may follow the change in a single el ment of the situation presented to the animal. Animal N 6, which manifested such fear and obstinacy when the keep carried a certain stick, never seems to associate blows fro the stick with the keeper himself. Nos. 3 and 8, which a always shut up at night in a small cage by themselve had to be driven in at the first with blows from a stic Now the association has been transferred to a mere moveme of the box, at which signal they spring to the door and claw open. No relation exists for them between keeper and box u til the box has first been moved from the wall.

A study of the number of errors made in learning Combin tion I, brought out a tendency which had not shown itself b fore. If the animal left off a series of trials with a certain nun ber of errors, and the intervening period was short, it wou in the majority of cases begin the next series of tests with a *le* number of errors than it was making at the end of the prece ing series. Two factors may have contributed to this resul 1. An unconscious elimination of errors during the interval no practice; 2. A positive development of the right orde the 1, 2, 3, 4, thrusting itself into the foreground until at la it gained the ascendency.

The raccoon seems to give a high grade of attention to th work in hand. This is of importance in the development of i associations and their permanence. Perfection of the order manipulation seems to come suddenly at last in spite of wh we know from the curves and otherwise of the framework associations which has been slowly set up in the midst of lon continued and apparently aimless fumbling.

Experiments on Color Perception.

There is excellent experimental evidence that monkeys ca discriminate colors as such. With raccoons the only exper ments as yet reported are those of Cole (10), which he himse regards as inconclusive upon this point. The experimen about to be described seem to me to indicate with a good deal of probability that the raccoon cannot discriminate colors as such, but depends on differences in brightness alone. His color blindness will perhaps seem less remarkable when it is recalled that the raccoon is the most nocturnal of all our mammals save the bats and flying squirrels, and that the work of Schultze and Ramon y Cajal has shown the retinas of nocturnal animals in general to be deficient in the retinal cones which seem to be essential for color vision.[1]

Apparatus. 1. A board 1″ x 10″ x 48″, had six holes bored on its centre line, 6″ apart. Six cylindrical tin cans, of uniform height and diameter, 4″ x 2½″, were fitted with plugs on the bottom, made to fit the holes in the base board, and thus prevent their being overturned. These cans were covered with Milton Bradley papers. Four were covered with the standard colors, red, blue, yellow, and green, the other two with neutral gray No. 1, and green gray No. 2.

2. The second piece of apparatus accomplished the same purpose as the first, by the use of six vertical slides, 6″ x 3″, with an opening 2″ x 1½″, cut at a uniform height from the base. These slides could be set in guides, in front of separate compartments 2½″ x 2½″ x 6″. The guides separated the slides about 2″ from each other. One set of slides was covered with the same papers as the cans of the first apparatus. A second set was covered with Hering's graded grays. This apparatus did not allow the animal to look into the container in which the food was placed.

The Milton Bradley papers were tested for brightness in a darkened room on a color mixer, using discs of black velvet and white cardboard as standard black and white. These determinations gave the approximate brightness value of red in terms of the white cardboard as 14°; green gray No. 2, 70°; blue, 82°; neut. gray No. 1, 140°; green, 167.5°; yellow, 240°.

The Hering grays of apparatus No. 2 were matched for brightness with the same apparatus as used above, but in diffused daylight. The black was given an approximate correction by use of Kirschmann's correction factor (*Wundt's Phil. Stud.*, Vol. 5, p. 300). These corrected numbers gave the series of approximate brightness values for the grays as 14.5°, 18°, 70°, 105.5°, 179.3°, and 360° respectively.

Method of Work. The two pieces of apparatus were used indifferently. The food was placed under one color, and when this color was moved to a new position, every other color was given a new position also. All changes in the apparatus were made out of sight of the animal. When the apparatus was set, it was so placed as to give the animal about three feet of free space for approach. In this series of tests the trials were made also under widely varying conditions of illumination, viz., morning, afternoon, twilight, cloudy days, and at night by lamp light. The sense of smell was fogged by strong essences, essential oils, and other means used by trappers to disguise the trail.

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[1] *Cf.* also Parinaud (27) La sensibilité de l'œil aux couleurs spectrals. The eye of the raccoon also agrees with the results of Slonaker (31) for other mammals below the primates in its lack of a fovea.

Results of Experiments in Color Perception.

Of the several thousand tests made on different animals, I have given in Tables III, IV, and V, a series of tests covering color experiments with two animals, and brightness with one, coons Nos. 1 and 2. The results in Tables IV and V were obtained from the same animal, coon No. 2. Each vertical column gives the number of times in thirty trials in which the animal went directly and without hesitation to the color named. For further abbreviation, gray in Table V represents the sum of all the choices of brightnesses other than black. These tables are, I think, characteristic of all results obtained by further tests on the same or different animals. They show nothing more than a qualitative tendency. No tests involving Weber's law have yet been made.

TABLE III.

Food in Green.

Red.	11	7	9	5	7	6	6	3	2	3	7	2	4	9	8	7	2	7
Blue.	3	1	2	6	0	9	4	9	5	10	8	4	5	6	6	3	4	2
Gr. Gray No. 2.	6	2	3	2	1	7	8	4	2	4	4	5	8	5	3	2	2	7
Neut. Gray No. 1.	2	6	7	3	6	4	4	6	5	3	2	6	0	1	2	4	9	4
Yellow.	2	8	7	10	2	0	1	1	8	1	4	5	6	4	5	8	1	3
Green.	6	6	2	4	14	4	7	7	8	9	5	8	7	5	6	6	12	7

TABLE IV.

Food in Red.

Red.	7	9	5	7	12	13	12	7	14	7	7	8	6	13	10	12	19	15
Blue.	2	1	4	4	3	6	2	6	2	2	3	5	8	0	7	6	1	0
Gr. Gray No. 2.	2	5	2	4	0	1	2	4	5	2	1	1	2	5	9	7	1	1
Neut. Gray No. 1.	8	4	4	6	7	6	9	7	0	2	7	9	5	2	2	2	4	6
Yellow.	3	6	7	5	7	2	2	2	7	9	6	4	6	3	1	1	4	3
Green.	8	5	6	4	1	2	3	4	2	8	6	3	3	7	1	2	1	5

TABLE V.

Food in Black.

Black.	11	23	13	22	14	17	20	20	26	29	27	29	28	26	29
Gray.	19	7	17	8	16	13	10	10	4	1	3	1	2	4	1

Where color and brightness enter as simultaneous factors in a series of tests as in Tables III and IV some confusion results. A comparison of the results from all the tests on color and brightness, in percentages (Tables VI and VII), shows a decided variation in favor of brightness. These tables summarize all the color and brightness experiments thus far made. It will be recalled, of course, that in the experiments with the brightness and also with the colors the animal should have chosen rightly by mere chance in about 17% of the trials. That coon No. 2 did better than this is probably due to brightness discrimination.

TABLE VI.

Showing Percentage Choices of Colors.

	No. Tests.	Right Choices.	Upper Limit.	Lower Limit.
Coon No. 1.	960	20.5%	43.3%	7%
Coon No. 2.	1020	40.7%	63.3%	23%
Coon No. 3.	600	16.5%	26%	8%
Coon No. 5.	450	18.4%	30.5%	12.5%

TABLE VII.

Showing Percentage Choices of Brightness.

	No. Tests.	Right Choices.	Upper Limit.	Lower Limit.
Coon No. 1.	690	45.5%	90.3%	16.6%
Coon No. 2.	600	65%	96%	36%
Coon No. 3.	420	59.7%	93.3%	10%
Coon No. 5.	360	39.7%	90%	30%

Profiting by the experience in discrimination of brightness when associated with color, the animals attain as shown in Table VII, a high level of discrimination when brightness alone is present.

Cole's experiments seem to confirm the results here recorded. His black-white tests give values superior to those tabulated by me, he having obtained as high as 100% in a fifty trial series. He displayed his test-colors pairwise, however, thus offering but two possible choices, while in my experiments the number of possible choices was greatly increased in that all six slides were exposed simultaneously. Thus if the tendency to discriminate should appear in the case of the six exposures, it must thrust itself up through a comparatively thick stratum of disturbing elements. In the case of the red-green tests one of Cole's animals made as high as 91½% of right choices, and another 83⅓%. He found, however, in this case as well as in that of the blue-yellow discrimination, that many more trials were necessary to attain successful discrimination than were necessary for a like discrimination of black-white.

A comparison of the Milton Bradley papers, used by Cole as well as by myself, according to the approximate determination of brightness given on p. 479, shows a brightness difference for blue-yellow, of 158°; red-green, 153.5°; black-white, 345.52°; black-yellow, 225.52°; black-blue, 67.52°; black-green, 153.02°. My own experiments show that in a rough way the raccoon can discriminate a difference of four degrees, and since all Cole's tests save one lie far outside this difference, I feel that his results give stronger confirmation of brightness discrimination than of color discrimination. Undoubtedly the raccoon can be taught to make what seem to be color discriminations, but it will really be a discrimination of brightness difference, which discrimination is essential to his wild life. He hardly needs color discrimination, since in the darkness of the night all colors fade to graded grays.

IMITATION.

When we approach the study of imitation in animal life we enter upon a field of controversy and of clashing definitions. It is necessary, therefore, that I state explicitly what I am going to mean by the word in the section that follows. I begin by excluding at the outset, all very accurate and nicely adjusted co-ordinate movements which bear the marks of inherited motor responses. If one of two puppies is thrown into the water and paddles his way to the shore while a second one looks on, we by no means have a case of imitation when the second puppy is likewise thrown in and also paddles ashore. And we can further exclude even those sorts of activity which are dictated by a strong gregarious instinct. We shall not in the present instance regard as imitation such actions as the general flight of a flock of crows when one or two of their number start off in alarm. For present purposes we shall regard as imitative those activities only which are individual in their nature and which are formed on the model of the acts of other animals. The action of the parrot which learns to talk, and the youth who goes to the Klondike in order, like others, to make his fortune, we shall regard as imitative. And we may distinguish two grades, illustrated tolerably by these two examples: a lower grade in which the imitation is executed for the satisfaction which it itself gives, and the other in which the imitation is undertaken with a view to the accomplishment of a definite result. The first of these is of an instinctive type and appears characteristically in many birds besides the parrot, in human infants, and possibly in the young of other species. The second is of a rational character, and when present is a mark of considerable psychic development. Here the issue seems fairly joined, for in raising the question—Does the animal on seeing a certain action performed by another desire to do the same in order that a certain result may follow?—we take our stand on the border line, howbeit somewhat broad, which separates rational from merely intelligent thinking.

In interpreting an action as imitative many investigators have placed great stress on what the animal *saw*, at the expense of what the animal *desired*. But even what an animal sees is by no means always certain, quite apart from physical imperfections in vision due to albinism, myopia, or nocturnal habits. Like Jastrow's "really do" and "apparently do," what an animal really sees and what it apparently sees may be widely different. Sufficient data from general observations are not yet forthcoming to certify the usual methods of testing imitation as fairly free from probable sources of error.

Citations will illustrate the point of contention. Berry (2) writes in regard to the rat:

> "When put into the box he first tries to get out at the place nearest to the food, but not succeeding there he gradually works away from that spot until he has tried almost every spot in the box, or until he pulls the string that opens the door. Then his attention being attracted to the door by the sound it makes in opening, he runs to it and passes out to the food. When he is put back, take notice of what happens. Does he run at once to the string, pull it and open the door? By no means. He tries the door and finding it closed makes many random movements before starting in the general direction of the string. After nosing about in its vicinity for a time he finally succeeds in finding it and pulling it and thus escapes. It is not until he has got out many times that he goes at once to the string, pulls it and passes out, without making at first a number of random movements."
>
> "These facts in regard to the way the rat learns to get out of a box are of vital importance in helping us to decide what we may reasonably expect from the rat in regard to imitation. If, when a rat by chance pulls the string and hears the door open, passes out and is fed, it cannot go directly to that string when put back, why should we expect a rat that has merely witnessed the performance to be able to do it. The rat that opens the door not only sees the string and sees his paw pull it, but he has in addition all the sensations that are connected with the movement of pulling the string, while the rat that looks on has only the visual sensation, no kinæsthetic sensations. It seems to me that we ought to be able to say *à priori*, in the light of these facts, that no ordinary rat would be able to open a door by pulling a string, simply from having seen another do it, without first making a number of random movements."

It is upon such a slender basis that Mr. Berry infers imitation. He lays great stress on the visual sensation as the chief factor in what he calls the final imitative act. It would seem as though he begs the whole question. If the act was imitation in any true sense, just what we should expect to see would be the rat pulling the string with much fewer random movements, else in what particular does No. 2's method of learning to get out of the box differ from that employed by No. 1? Indeed, it would seem to require more than the "ordinary rat" to acquire the ability to free itself from the box, if it had no other tutelage than seeing another rat do the same thing. Small (32) expressly states, that he has never seen a case of what might be called inferential imitation. "By this I mean," he says, "merely the learning to do a thing by seeing another do it—the purposive association of another's action with a desired end."

Cole (10) again writes in regard to his raccoons:

> "After having had some six weeks of experience in distinguishing a black from a white card and in distinguishing complementary colors, each of the four raccoons developed a tendency to reach over the front board of the apparatus and claw up the colored cards. This tendency was encouraged and finally they would claw up the right (food) card and go to the high box to be fed, or, having clawed up the wrong (no food) card they would claw it down. So far as imitation is in question, the important point is that the raccoons did begin to do, or try to do what they had seen done by the experimenter. Before they began this they had learned to watch the cards and the movements of the trainer's hands very closely indeed. Therefore, the animals either imitated or else from their impatience to see the right card come up there sprang the idea that they themselves might make it come up. This, however, may be all there is in intelligent imitation."

To the present writer it seems that there is yet another alternative in this case. The behavior of the raccoons in clawing up the cards was due either to imitation, or to the idea that they themselves might make the proper card come up, or *it was an accidental result of the raccoons inveterate impulse to attack and manipulate anything that can be moved.* The animals had already associated the colored cards with the getting of food; had earlier still been accustomed to get food by attacking some sort of fastening; from this it is a short step—hardly a step at all—to attack the card holders. After having succeeded a few times in thus starting the train that leads to feeding, the activity would become stereotyped like the opening of boxes or any other. In any case we might expect imitation to occur, if at all, more readily when members of their own species furnish the models; and while these cases remain few and uncertain we may well entertain reserve as to those where the coon seems to imitate the experimenter.

As to the imitation of one coon by another, though I have been alert to discover it, I can report only doubtful or negative observations. Coon No. 3 sits up and begs for food (Fig. 11, Pl. II). She took up this activity of her own accord. I have simply strengthened the association of food and sitting up, by refusing food unless she does so. At the first No. 8 used to sit up also. They took up the attitude together. Soon No. 8 dropped it and then picked it up again about three weeks later. Now he sits up regularly. No. 2 and No. 4 were taught to climb to the top of the stand in the middle of the floor of the cage for their food. No. 4 was taught to stand up on this stand for his food. He is blind so no visual element could have entered into his learning of the trick, but No. 2 which climbs to the stand with him, never stands up for food unless it is held directly above her head, when she will reach for it. No. 2 has had good opportunity to observe and therefore, copy, but no imitation has resulted. It might perhaps be thought that the climbing of the stand was the beginning of the imitative process, but No. 2 has been accustomed to climb to my shoulder for her food for more than a year. Climbing to the stand was therefore, simply a change of place. In the cases of No. 3 and No. 8, the position which they assume is one which is natural to the animal when suddenly startled. It then sits up to look for the cause of the disturbance. The position is also a preliminary fighting attitude. Both animals are timid and have not been long in captivity. It would seem, therefore, that we had here merely a partially transferred association from that of —danger, sit up—to that of—food, sit up.

No. 6 has taken to himself the habit of jumping for his food. This activity has been fostered, until he now jumps about 3 ft. high for his morsels. He is in the midst of the pack when they are being fed. No other animal has yet copied his method, although he rubs against them and jumps from their midst continually.

Six animals escaped from the cage in one night, by pulling down the wire netting in a corner at the ceiling. Three of these belonged to one litter; a mother and her two young were the second three. Once out of the cage, the first three jumped from the window to the ground a distance of some sixty feet. Two escaped, the injured one was recovered. The other group concealed themselves in one of the ventilating shafts in the wall of the building from which they were extricated with difficulty. Three other animals remained in the cage and made no apparent attempt to escape. The case of the six that escaped might be considered as indicating something of imitation, but I think it rather a case of the group or gregarious impulse to which I have already referred as of a different sort from the strict type of imitation under consideration. We know as yet so little of the importance to animals of litter and species odors, and of the general basis of gregariousness, that this factor must not be lost sight of.

Excluding such cases as this last and the doubtful ones of defecation and play already referred to (p. 456 p. 460) above, I have seen nothing that I thought I was justified in regarding as a clear case of imitation, especially none of the inferential type. At the same time I am far from believing that imitations of the more instinctive type may not yet appear, especially in the case of young animals and of raccoons when free at night.

SUMMARY.

In summing up the results of the observations and experiments reported above the writer feels justified in making the following statements:

1. The common opinion of the adroitness and cunning of the raccoon is well founded and in agreement with his habits in the wild and with what may be inferred from his physical equipment and great adaptability.
2. The raccoon's instinctive powers of attention and curiosity are of a high order as compared with those of other animals.
3. The coons under observation developed a characteristic of depositing their excrement in a fixed place and covering it there, a form of behavior to which they are not accustomed in the wild.
4. The raccoons under observation showed wide differences in temperament—differences which are fundamental in the interpretation of all tests on their intelligence.
5. The experiments in which the animal opened various locking devices in order to obtain food showed a considerable variety of attack in the case of different animals and to a certain extent in the same animal at different stages. The perfecting of the power of undoing fastenings is accomplished by a slow series of small changes, consisting chiefly in the omission of unnecessary movements and the combination of those required.
6. The order of procedure in working the "combination" locks was perfected before the amount of effort necessary was fully learned.
7. Experience with former fastenings holds over in the case of new ones leading the animal at least in certain cases to begin his attack at the place on the surface of the food box where he has been accustomed to work. (This has been found by Thorndike in the case of cats, but denied by Cole in the case of raccoons.)
8. When an animal is forced to approach a familiar fastening from a new direction, it is often about as much bothered by it as by a new fastening. Nevertheless in course of time the animals seem to reach a sort of generalized manner of procedure which enables them to deal more promptly with any new fastening (not too different from others of their experience) than with the first few which they mastered.
9. The old animals seem to learn less rapidly at first than

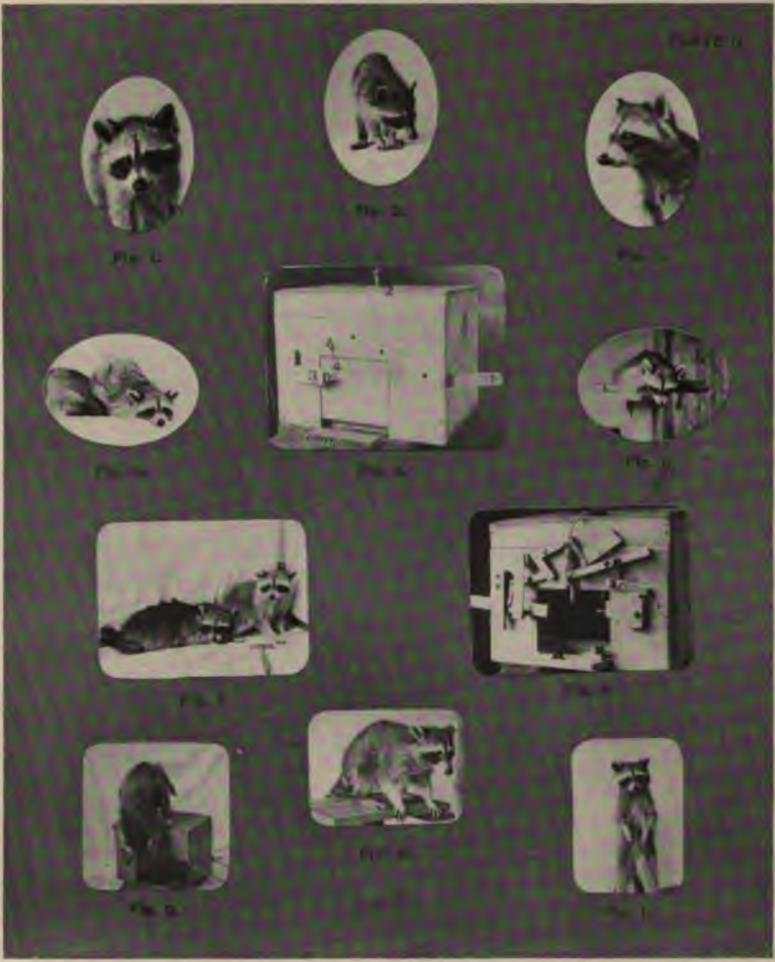

PLATE II.

Figs. 1, 2 and 3 show animals No. 4, 6 and 2, in characteristic attitudes of attention.

Fig. 4. No. 3 in the attitude of fear.

Fig. 5. Combination No. 1 closed and set, ready to be given to the animal.

Fig. 6. No. 2 in a characteristic resting or sleeping attitude.

Fig. 7. No. 8 at the left in the attitude of fear, and No. 2 at the right ready to fight.

Fig. 8. Internal mechanism of Combination No. 1, showing the interplay of locking devices.

Fig 9. No. 1 in the act of opening 3 and 4 in Combination No. 1.

Fig. 10. No. 2 in the attitude of attention.

Fig. 11. No. 3 begging for food.

the young, but profit more by their experience later, possibly because they are more attentive to the matter in hand and are more economical in the matter of effort and movement. These age differences appear in the learning curves.

10. The learning curves for the raccoons and Kinnaman's monkeys, so far as they permit a comparison of the animals, seem to show a nearly equal facility in learning to undo fastenings.

11. Test of the raccoon's powers of retention show that skill in undoing simple fastenings once learned remains practically undiminished for periods of no practice of more than a year. In the case of the "combination" locks the memory was imperfect after a period of 286 days, but the relearning was rapid, only 24 trials being necessary to gain a facility that originally required 107.

12. There was evidence in certain cases, where the intervals of practice were relatively short, of a gain in facility during the period of no practice, so that the animal began with fewer mistakes after the interval than before. This may, however, have been due to the beneficial effect of rest since the animal usually went to sleep at once after the tests.

13. The animals after practice adapt themselves correctly to comparatively small differences in the situation with which they are confronted.

14. The experiments on the discrimination of colors and brightness indicate with extreme probability that the raccoon is color blind. The raccoon's nocturnal manner of life would lead the experimenter to expect this result.

15. No certain cases of imitation of one raccoon by another were observed. The two or three doubtful cases were on the instinctive, not upon the rational level.

I wish here to thank Dr. E. C. Sanford for his kindly encouragement, suggestions and criticism during the entire period of observation. I owe acknowledgment also to Mr. J. H. Starr for assistance in the capture of the animals, and observation of the habits of animals in the wild which were fresh and first hand.

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MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

Communicated by Edmund C. Sanford.

XIX. A Preliminary Study of the Psychology of Reasoning.

By Willis L. Gard.

An examination of the treatment of reasoning by most psychological writers reveals the fact that they have been largely influenced by the demands of formal logic. For many centuries the syllogism was regarded as the only form of reasoning. So recent a writer as John Stuart Mill enters a vigorous protest against the attempt, on the part of a few, to look on the syllogism as useless in reasoning. For him the syllogistic form is absolutely indispensable in testing the correctness of generalizations. But he admits that the syllogism is not a correct analysis of the psychic process of reasoning.

According to Mercier the regard for the syllogism, as a form of thought, has declined during the last thirty years. He affirms that the study of the reasoning processes should form a part of psychology; but he admits that in his discussion of the matter he has "bowed the knee in the House of Rimmon."

Lloyd Morgan declares that only those beings reason that are capable of "focusing the therefore," and implies that reasoning is a conscious attempt to justify our conclusions. It is an examination of the method by means of which we reach a conclusion. William James is still willing to say that "reasoning may be very well defined as the substitution of parts and their implications or consequences for wholes," though he treats the matter at some length descriptively.

Binet finds that there are three images involved in reasoning. The first calls forth the second by resemblance. The second suggests the third by contiguity; and this is all that there is in reasoning. Ribot would have us believe that "reason is only a means for control and proof."

George M. Stratton, however, writing in the *Psychological Review* for 1896 maintains that both logic and psychology have an interest in reasoning. But in the past the part that belongs to each science has not been very well made out and much confusion has resulted. Each has a different end in view and employs a different method. In brief, Stratton holds, and rightly, that in logic, there is an attempt to justify the conclusion; in psychology there is an attempt to explain how the conclusion was reached.

These few and brief citations will serve to show how the discussion of reasoning has been influenced by formal logic even in the case of recent writers on the subject.

But little attention has been given to an experimental study of the question as yet. This may be explained in part by the fact that sensation and perception have furnished the experimentalist easier points of attack. Indeed, some have gone so far as to assert that reasoning is such a very complex process that we have little or no chance of ever being able to catch the mind in the act of pure reasoning. Groos, who has made some attempt in this direction, thinks that perhaps we shall be able to get most empirical material by noting the blunders of reasoning into which we fall. That reasoning is a difficult matter to deal with empirically is certainly evident to all, but it does not seem fair to place it among the impossible problems without further consideration.

The writer confesses himself more hopeful of a solution of these problems by the empirical method. Certain phases of reasoning may be observed in the solving of all problems and puzzles, and by introspection it ought to be possible to gain a fair account of the psychic process involved in the reasoning act. Believing that some information concerning reasoning could be obtained in this way, two series of simple experiments have been carried out.

THE FIRST SERIES OF EXPERIMENTS.

The first set of experiments was made with a problem in long division, found in the "puzzle column" of the *Congregationalist*.

The problem is as follows:

```
9 x x ) 4 x x x 4 x 7 ( x x x x
        x 9 x x       ---------
            x 1 x x
            4 x x 5
            -------
              2 x 7 x
              x x x 4
              -------
                x 0 x
```

It was the task of the subject to supply the proper figures for the crosses, and to take note, so far as he was able, of his mental processes while doing so. These introspections were recorded by an operator who was present during the solution. In this way it was possible to secure a somewhat full account of what went on in the mind during the solution. The problem was solved in this way by twenty-six persons.[1]

The following imaginary record is typical of the subjects, method of procedure except that the actual ones were less certain and direct. All notes and introspections also are here omitted. The procedure may be followed in the example below where the large figures are those of the original problem, the small figures above them are the numbers of the positions of the individual figures and x's numbered consecutively from left to right, and the letters below indicate the successive steps of the solution.

```
 1  2  3     4  5  6  7  8  9 10    11 12 13 14
 9  x  x  )  4  x  x  x  4  x  7  (  x  x  x  x
    n  h        r  r  r     f        p  g  h  j
    o  l  ) 15 16 17 18           (  q     i  k
             x  9  x  x
            ---------------------
                  19 20 21 22
                   x  1  x  x
                   m     b  e
                  23 24 25 26
                   4  x  x  5
                  ---------------
                     27 28 29 30
                      2  x  7  x
                               a
                     31 32 33 34
                      x  x  x  4
                            d
                     ------------
                        35 36 37
                         x  0  x
                               c
```

a. Inserted 7 in *30* from data given.
b. Inserted 4 in *21* from data given.
c. Obtained 3 in *37* by substraction.
d. Obtained 7 in *33* by substraction.
e. Obtained 2 in *22* by general rule of subtraction.
f. Inserted 2 in *9* from data found in fifth step.
g. Inserted 0 in *12* from data found in *21* and *22*.
h. Decided that *3* and *13* must be odd because *26* is odd.
i. Decided that *13* must be 5 because *23* is 4.
j. Decided that *14* must be 2 or 3 because of *27*.

---

[1] The writer wishes to express his obligation to Dr. Alvin Borgquist for the notes upon these solutions which were taken by him under the direction of Dr. Sanford.

k. Decided that *14* must be 2 because *3* is odd and *34* is even.

l. Decided that *3* must be 7 because it is the only odd number that multiplied by 2 and 5 gives 4 and 5 in units place in the product.

m. Decided that *19* must be 5 because *24* is greater than 1.

n. Decided that *2* should be 3 or 8 by alteratives.

o. Decided that *2* must be 8 by trial and error.

p. Decided that *11* must be 4 or 5 because of *4*.

q. Decided that *11* must be 5 by trial and error.

r. Inserted 9 8 6 in *5 6 7*.

A few other relations were used by a few of the subjects. The most important is the discovery that the 24th figure is 9.

The subjects tested in this series were of very various degrees of skill in introspection and it is quite clear that most were unable to do more than indicate the steps taken without entering upon a minute account of them. This is not at all remarkable, however, for there was apparently nothing but the associative rise of the required numbers in consciousness that could have been observed. The writer's own experience with the problem, confirmed by that of the best expert subjects that have been tested, seems to show that the reasoning processes involved are matters of simple associative recall. The particular feature of the problem which engages attention at the moment calls up associatively the digit required, or, when the case is more complicated, the several possibilities. *No trace appears of anything other than the familiar processes of association and apperception working under the special conditions of attention to the particular matter in hand.*

Of more importance perhaps than the elementary functions involved in reasoning, is the general method of attack upon the problem presented, the way in which clues are looked for and the adequacy with which they are apprehended when found. Here lies an immense field for individual differences. After tabulating the solutions of the problem, it was found possible to group them with regard to efficiency in three classes. The basis for the classification is the number of steps required to reach the solution and for this reason is arbitrary.

The best group consisted of seven solutions: 2 requiring fourteen steps; 1 fifteen; 1 sixteen; 1 seventeen and 2 eighteen. The medium group consisted of eleven solutions: 2 of nineteen steps; 3 of twenty; 2 of twenty-one; 3 of twenty-two and 1 of twenty-three. The poorest group consisted of eight solutions: 2 of twenty-four steps; 1 of twenty-five; 1 of twenty-six; 1 of twenty-eight; 1 of thirty-four; 1 of thirty-five and 1 of forty-nine.

The difference in the number of steps required to solve the problem shows the individual differences. It is no doubt true that the variation in the number of steps may be explained in part by the difference in completeness with which the introspections were made. But some of the variation must certainly be attributed to psychical factors, such as methods of association, sagacity, and familiarity with the particular numerical relations involved. It is to be noted, however, that those most skilled in mathematics did not always reach the solution by the use of the fewest steps.

The different groups show different amounts of mental fumbling. There is, as might be expected, a close relation between the amount of fumbling and the number of steps required since the fumbling, or inability to see essential relations clearly, is the chief cause of the lengthening of the process of solution. When the successive steps of the solution are treated in groups of five the earlier groups show a higher percentage of fumbling than the later ones. There is much fumbling until a cue is found, which enables the subject to take a few definite steps. This, however, is soon exhausted and there often must then be another search with renewed fumbling. Only a few relations are simultaneously before the mind.

The method of attack seems to be influenced by habit. Five out of seven in the best group of subjects began with the first figure of the quotient. Seven out of eleven in the second group began with the same figure, while only three out of eight in the third group began with this figure. These facts show a rather strong tendency to follow the fixed habits in commencing the solution, and a certain advantage in so doing, or in the methodical habits of procedure that it indicates. They begin at the point where one would in solving an ordinary problem in division. It is in the formation of such habits that growing skill in mental operations consists.

THE SECOND SERIES OF EXPERIMENTS.

In the second series of experiments the material was of the same nature as before, but the problems were much simpler. The form of each problem follows:

I.	II.	III.	IV.
xx7)2077(2	8	x)x64	6x
xxx4	x	124	x
103	5		594
	18		

$$\begin{array}{ccc} \text{V.} & \text{VI.} & \text{VII.} \\ \begin{array}{r} 7\,6 \\ \underline{5\,x} \\ 1\,7 \end{array} & \begin{array}{r} x\,)\,\underline{x\,6\,x\,6} \\ 1\,9\,2\,4 \end{array} & \begin{array}{r} x\,7\,4 \\ 2\,x\,4 \\ \underline{3\,5\,x} \\ x\,5\,3\,2 \end{array} \end{array}$$

The task in these problems was the same as in the previous one. In all twenty persons have solved these seven problems and given a rather full account of the mental processes taking place during the solution. These subjects were all more or less experienced in introspection. An operator was present at the time the work was done and secured as full an account of the work as possible. The direct reports as to the detail of the processes was as meagre as before, but the simpler conditions permit more certain inferences as to its character. For an indication of the character of the data obtained, see pp. 499-501 below.

On examining the results, it was found possible to group the solutions according to the following scheme.

TABLE I.

Problems.	I.	II.	III.	IV.	V.	VI.	VII.
Sub- 1	B	A	E	B	C	D	C
ject 2	B	A	E	B	C	E	C
" 3	A	A	D	A	A	D	C
" 4	B	A	E	B	C	E	C
" 5	B	A	E	B	C	D	C
" 6	B	A	E	B	C	D	C
" 7	B	C	D	B	C	D	C
" 8	B	C	E	B	C	E	C
" 9	B	A	E	A	A	E	C
" 10	B	A	E	A	C	E	C
" 11	A	A	D	A	C	D	C
" 12	B	A	D	A	A	D	C
" 13	B	A	E	A	C	E	A
" 14	B	A	E	A	C	E	C
" 15	B	A	E	A	C	E	C
" 16	A	A	D	B	A	E	C
" 17	A	A	E	A	A	E	C
" 18	B	C	E	B	C	E	C
" 19	B	C	E	B	C	D	C
" 20	B	A	E	A	A	D	A

Explanation of letters used in classifying the solutions.

Problem I.

A = The use of a general principle. This involved the subtracting of the remainder from the dividend and dividing by the quotient.

B = Trial and error method. The attempt to supply the missing figures by comparing the quotient and the divisor. The process was not wholly dependent on chance, but involved more or less uncertainty.

Problem II.

A = The use of a general principle. This consisted in adding 5 and 8, and subtracting the sum from 18.

C = The missing figure was supplied by mechanical sustitution.

Problem III.

D = The problem was solved by using one variable. The divisor was used as multiplier of the quotient. The divisor was changed until a product was obtained with a 6 in the ten's place.

E = The problem was solved by using two variable terms,—the divisor and the first figure in the dividend.

Problem IV.

A = The use of a general principle. This was done by dividing the product by the multiplicand.

B = The trial and error method. This consisted in assuming values for the missing figures and changing them until the conditions were satisfied.

Problem V.

A = The use of a general principle. The remainder was subtracted from the minuend.

C = Mechanical substitution as the result of familiarity with the relations involved.

Problem VI.

D = The use of one variable.
E = The use of two variables.

Problem VII.

A = General principle used. See A under the second problem.
C = Mechanical substitution. See C above.

Methods *D* and *E* stand between methods *A* and *B*, in that they are guided by general principles though not such as can be so easily formulated and brought definitely to consciousness, and even in the *B* method the trials are not wholly at random.

Summary of the table.

Problem I.	Problem II.	Problem III.	Problem IV.
A = 4	A = 16	E = 15	A = 10
B = 16	C = 4	D = 5	B = 10

Problem V.	Problem VI.	Problem VII.
A = 6	D = 9	A = 2
C = 14	E = 11	C = 18

Problem I was to many rather difficult, and the results show a very strong tendency to use the "trial and error" method in the solution. This method was persisted in in spite of the fact that a very direct method was plainly indicated by the data given. The form of the problem seems to have suggested old associations and the subject at once began the search for the cue by using the quotient as one factor. It was difficult for most to change this first impression and often a struggle resulted. Those who applied the general principle did so at once and the result followed immediately.

The second problem was an easy one and these subjects show a strong tendency to use a general principle in its solution. The solution followed almost as soon as the problem was perceived, but fifteen out of twenty testified that a solution was reached by applying a general principle. Possibly, however, the general principle may have been appealed to for justification of the result to themselves rather than in reaching the solution.

The third problem was more difficult, but there was an increase in the number of those who attacked the problem in the most direct way. In this problem and in the sixth one the most direct method was to use but one variable quantity. It is noted that there is an increase in the number of those who saw the most direct method of solving this problem. This may be due to a "warming up" to the work, to the influence of practice, or to the refreshing the mind for the old numerical relation.

The fourth problem was selected because it presented conditions of medium difficulty. The subjects fall into two equal classes. Those who used the *A* method applied the most general principle possible to its solution. Those who used the *B* method followed the "trial and error method." They varied the two missing figures until a set was found that satisfied the conditions.

In the fifth problem fourteen out of twenty found themselves applying a general principle of subtraction. They subtracted the remainder from the minuend and thus obtained the missing figure in the subtrahend. Some felt sure, however, that the missing figure was supplied almost spontaneously on seeing the problem.

The seventh problem contained very simple conditions and the results show that eighteen out of twenty saw the missing figures instantly on seeing the given figures in each column. These eighteen assert that there was no adding of the given figures and subtracting this sum from the grand total. The associations were so ready that they resulted at once on seeing the conditions.

Some General Observations Made by the Subjects.

One subject reports that a solution which starts off with success but breaks down at a point near the end, produces something like a mental "cramp." The mind is reluctant to abandon a course that has almost resulted in success. There is a tendency to believe that the data are wrong rather than admit there is an error in the computation. There seems to be a poverty of possible chances, and a tendency to force a solution along lines that give an initial success. The same person remarks that he tries to solve puzzles by trying the same plan time after time. An initial success which proves to be partial makes it all the more difficult to modify a plan of attack, while initial failure leads more readily to new plans.

The fourth problem is as follows :

$$\begin{array}{r} 6\ x \\ x \\ \hline 594 \end{array}$$

One subject noticed that the fact that a six was placed in ten's position in the multiplicand caused him to conclude that the missing figure in the multiplicand cannot be a six. The mere presence of the six had excluded the possibility of the missing figure being a six. He had a feeling that the missing numbers must be eight and eight, and it was difficult to drive himself into an admission that the missing numbers must be nine and six. The thought of putting another six in the multiplicand was certainly inhibited by the presence of one six already in the multiplicand. Several others noticed that they were led to believe that the missing figures in the multiplicand and multiplier were alike. The fact that each was represented by the same sign seemed to suggest this to them, although they had been told at the outset that a cross may represent any one of the digits.

One subject reports that the large product in the fourth problem suggested a large digit for the multiplier. The subject was conscious that the 594 was very near to 600, and because of a tendency to work with ten as a unit the multiplier was seen to be 9.

Another testifies that the missing figures in the fourth problem were kept in the foreground, while the 6 and 59 were in the background.

Another reports that he tried 6 in the multiplier of the fourth problem because he felt that the multiplier must be a reasonably large number. The six was taken before he had considered all the data, and the conclusion seems to have been based on a vague consciousness of the relations existing between the given factors.

One person in solving the third problem asserts that he did not try a large number in the divisor at first, for the product obtained by multiplying the divisor by the quotient must not contain more than three figures. He must keep within this limit.

It may be suggested that the figures with which the subject began give a fair indication as to his general feeling regarding the numbers to be supplied.

The following will illustrate the nature of the data gathered:

The A Method in the First Problem.

The subject first saw that the second line contains blanks and at once resorted to a general rule of subtraction— "Given the minuend and the remainder to find the subtrahend." As soon as the subtraction was completed he knew that the second line was twice the divisor. This division gave the required divisor.

The B Method in the First Problem.

$$\begin{array}{ccccccccc} \overset{1}{x} & \overset{2}{x} & \overset{3}{7} & \Big) & \overset{4}{2} & \overset{5}{0} & \overset{6}{7} & \overset{7}{7} & \Big(\overset{8}{2} \\ &&&& \overset{9}{x} & \overset{10}{x} & \overset{11}{x} & \overset{12}{4} & \\ \cline{5-8} &&&& & \overset{13}{1} & \overset{14}{0} & \overset{15}{3} & \end{array}$$

The first idea was that the figure in position (1) may be 1 or 9. But it was soon decided to reject 9 on the account of 2 in position (4). At this point it was discovered that 2 was the entire quotient. The problem was begun anew by multiplying seven in position (3) by 2 in position (8). Since the figure in position (11) is 7, the figure in position (2) must be such that when multiplied by 2 will equal a number ending in 6 for a product. This will make the figure in position (2) equal 3. Then the first in the divisor is a 9 after all. Then 9, 8, 7, 6, 5 and 4 were tried as the second figure of the divisor, but all were rejected and 3 was declared once more to be the correct number for the position. But this does not work. At this point the subject was forced to repeat the series 3, 4, 5, 6, 7, 8 for position (2). By this repetition 8 was found to be the correct figure. In the first trial 8 seems to have been skipped in some way.

The A Method in the Second Problem.

The subject saw at once that the process was addition. Immediately he saw $8 + 5 = 13$. $18 - 13 = 5$. The first step came without hesitation and then there was a slight delay in the subtraction process, due perhaps to the verifying of the results.

The C Method in the Second Problem.

The sum was seen to be 18. $5 + 8 = 13$, and at this point saw that the number required equaled 5. He did not say "13 from 18" to get his 5.

The D Method in the Third Problem.

The subject began with 24 in the quotient and the six in the dividend. He noted that the first multiple of 24 containing 6 in the ten's place was 7. $7 \times 24 = 168$. Having done this he concluded that the divisor was 7. The entire dividend was then obtained by multiplying 124 by 7. It required only a few seconds to find the blanks, and the subject began at once his solution with 24 and 6 as the clew to the solution.

The E Method in the Third Problem.

The subject began by saying x into x will equal 1. But this told him nothing. He then looked at 6 in the dividend and 12 in the quotient and repeated $12 = 2 \times 6$. He dropped this clue as it did not amount to

$$\begin{array}{r} x \;)\; \underline{x\;6\;x} \\ 1\;2\;4 \end{array}$$

anything. He then saw that six in the dividend was the clew. 2 into 6 equals 3. From this he decided that 3 was the divisor and three was the first figure in the dividend. Here he was balked. He then tried 4 in the divisor. Impossible to say just what was done here. He lingered over this point for some time. He then multiplied 124 by 2 to see if it would not help; but it would not do. He had already rejected 3 from the divisor. He then returned to the divisor and tried 2 once more forgetting that it had already been rejected. He then tried dividing 26 by 2 with 13 as the quotient. This did not help. He tries 3 in the divisor once more. "Must have felt that I made a mistake before." He then realized that he was trying each digit in succession. He now tried 4 but soon rejected it. He now felt that he did not want to follow up the scale any further. But at the same time this would give the correct result. He now returns to 124 in the quotient and multiplies it by 4. He was now ready to try anything to get a change of attack. He returned to 3 for the divisor once more, being influenced by the 2 in the quotient and the 6 in the dividend, although this had been worked out and the 3 rejected from the divisor. He now saw that 124 in the quotient would have to be multiplied by something so as to have 6 in the ten's place as shown by the 6 in the dividend. He then tried 5, 6 and seven as multipliers. 7 gave the desired result.

The A Method in the Fourth Problem.

As soon as the subject saw the data, he divided 594 by 6. This gave 9 for the multiplier. Then 9 × 60 = 540. 594—540 = 54. 9 into 54 = 6, the missing figure in the multiplicand.

The B Method in the Fourth Problem.

The subject's first thought was that the missing figures must be alike and that 8 × 8 would satisfy the conditions. He had a feeling that the missing figure in the multiplicand must not be 6, for the 6 already in the multiplicand closed the door against the possibility of another 6 in the multiplicand. But 8 × 8 gave 64. This would not do. With 9 in the multiplier and six in the multiplicand the product would be 54. This showed that he must have 5 to carry. He then thought of 10 × 6. But he could scarcely get away from the 8 × 8. He was then pushed into 9 × 6 = 54. The 6 certainly inhibited the placing another 6 in the multiplicand.

The A Method in the Fifth Problem.

The subject saw at once that the process was subtraction. $\dot{7}6 - 17 = 59$. The method was suggested just as soon as he saw the data.

The C Method in the Fifth Problem.

The subject sought for a number which when subtracted from 16 would give 7 for a remainder. He saw that the missing number must be larger than 6 to give 7 for a remainder. He then added 10 to 6. Then he sought for a number which subtracted from 16 would equal 7. This number was 9. But he was not satisfied with 9 till the subtraction was completed.

The D Method in the Sixth Problem.

The process is division by a single number. There is no remainder. To secure 4 in the quotient the last 6 in the dividend must become a 16 by carrying one ten. Then 4 is tried in the divisor. The clue was taken from the last figure in the dividend and the last figure in the quotient. The problem was solved backward.

The E Method in the Sixth Problem.

The subject saw that he must have a large remainder after the first division in order to have 9 as the second figure in the quotient. He first tried 3 for the divisor and 5 for the first figure in the dividend. This would not do. He then looked at the end and saw the divisor must be an even number or something that would divide a number ending in 6 evenly. He rejected 5 at once for the divisor. He then tried 4 in the divisor and 7 in the first place of the dividend. This verified.

The A Method in the Seventh Problem.

The subject saw that $4 + 4 = 8$. $12 - 8 = 4$. $5 + 7 + 1 = 13$. $13 - 13 = 0$. $3 + 2 + 1 = 6$. $15 - 6 = 9$.

The C Method in the Seventh Problem.

The subject saw at a glance that the missing figure in the first column must be 4. He said 4 + 4 are 8 and 4 = 12. Then said 7 and 5 and 1 = 13. The missing figure in the second column is 0. Then added the last the same as the first.

In a general way the solutions are upon three levels.

1. *Direct substitution.* These subjects were so familiar with the different numerical combinations that the missing figure was brought to the front as soon as the data were apperceived. Some may wish to classify this as memory. But is it? In all probability this was the first time that these particular figures were ever given to the subject in exactly this relation. To the writer's mind it is the simplest or limiting case of reasoning. The series from it to the more complicated cases is an unbroken one, and the latter are, so far as I can discover, but compoundings of such simple associations. In this group come those solutions marked *C*.

2. *General Principle.* These subjects were not so facile with the mere numerical relations and were unable to reach the missing figures by direct association. They saw clearly enough the general principle involved but the supplying of the data had to take a slower course. The associative processes operated within limits imposed by the "general principle" apperceived. In this group come those marked *A* and *D*.

3. "*Trial and Error.*" The subjects using this method saw the relations less perfectly. They were not consciously guided by general principles but set about fulfilling the conditions empirically. The solution was often delayed and frequently the steps were repeated. In this group are found those solutions marked *B* and *E*.

It should not be concluded that the general reasoning powers of the subjects themselves are fully characterized by these tests. For these groups do not represent stages of development The type selected for the solution of a problem was determined largely by the subject's familiarity with the conditions given. Perhaps all use the method of "trial and error" when a difficult problem is first attacked. DuBois-Reymond quotes Helmholtz who thus describes his mental processes in solving mathematico-physical problems:

"I must compare myself to a mountain climber, who, without knowing the way, mounts slowly and painfully. Often he must turn back for he can go no further. Sometimes through intuition, sometimes through accident he discovers a trace of a new way. This leads him forward again for a short distance and finally he reaches his goal. Then he discovers to his shame a royal way upon which he could have come, if he had only been clever enough to find the correct beginning."

General Conclusions.

1. The psychological process of reasoning as shown by these experiments does not follow the form of the syllogism, which is not even frequently employed to test the correctness of the conclusion. This is by no means to say that much, or even all, of the reasoning employed resists restatement in syllogistic form. The logical forms are like mathematical symbols in the range of their applicability and also in their abstract and unpictorial character. As well expect to find in 3.14159 a concrete picture of a circle and its diameter as to find in a syllogistic statement a reconstruction of the psychic processes that it symbolizes.

2. The processes are, so far as could be discovered, the familiar processes of association and apperception working under the special conditions of attention to the particular matter in hand.

3. Familiarity with the relations results in quicker solutions and in solutions that are more direct.

4. The established habits of procedure have an influence on the method of attack. This was so strongly marked in some cases as to cause a long use of the "trial and error" method when a general principle was plainly indicated by the data given. Dr. E. H. Lindley found a similar result in the case of children in his "Study of Puzzles."

5. The almost successful solution of a problem often produced mental "cramp." Because the solution was so nearly correct, the mind refused to try a new plan or to make the change of a single figure.

6. There is evidence for believing that reasoning is often an obscure insight or only a guess with an appeal for assistance to the method of "trial and error."

7. Some subjects stated that there was present a vague back-

ground which guided the associations. In some instances the general notion in the background was wrong, and this led to wrong procedure and to a long delay in the solution.

But what is the nature of this "background" or "mental set," and what influence has it on the reasoning process? Is the "mental set" anything more than a neural state brought into activity by seeing the problem? Perhaps we could say that there is a general arithmetical "principle" persisting in the "background" which determines the associations, but again, what is the nature of this general "principle?" Can it be anything more than a general habit of attention under certain circumstances?

The same problem may be put in this way. Reasoning may be defined as a series of successive limitations put on attention and association. But once more, what factors determine this process of limitation? We may say, in general, that it is determined by two things: By something *without*,—in this case the figures arranged in such a way as to form a problem. By something *within*,—the general knowledge of arithmetic and numerical relations and the momentary interest in the solution of the problem. These two influences acting jointly determine the "mental set," which in turn controls our attention and decides what course the associations shall take. But even this does not give us the final word regarding the nature of the "background" or "mental set." For the present this question must be left open.

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XX. ON THE READING AND MEMORIZING OF MEANINGLESS SYLLABLES PRESENTED AT IRREGULAR TIME INTERVALS.

By MARGARET K. SMITH, Ph. D.

The natural tendency to read disconnected syllables in rhythmic groups and the comparative ease of memorizing rhythmic material are matters of common observation. In the laboratory studies made with meaningless syllables, it has been found practically impossible to avoid metrical groupings even when the syllables themselves are presented in strictly uniform series. A subjective tendency toward grouping forces upon them a metrical form which does not exist in their objective arrangement. During the writer's experiments upon Rhythm and Work[1] certain tests were made with the reading of meaningless syllables, in the course of which the question arose as to the extent to which objective inequalities may exist and a rhythmic grouping still be maintained, and, as a secondary question, what the rhythm secured under such difficulties signifies to the individual.

With a view to answering the above questions in connection with voice rhythm, at least, a series of experiments was made in the Psychological Laboratory at Clark University, Worcester, Mass., in the winter of 1900–1901, and though the experiments were unsuccessful as regards finding any order of syllables so irregular as to be wholly refractory to the rhythmic impulse of all the subjects, it seems worth while to give a brief account of them[2]

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[1] Rhythmus und Arbeit: Wundt's *Philos. Studien*, Bd. XVI, 1900, 71-133.

[2] The work was under the general direction of Professor Sanford, to whom and to President Hall, the writer wishes to express her sincere thanks for many helpful suggestions, as well as for the many privileges of the laboratories, the library, etc.

I. Apparatus and Method.

The material consisted of meaningless syllables, while the procedure consisted in reading aloud these syllables as they appeared at irregular intervals on a horizontal rotating cylinder, similar to that of the Baltzar kymograph. The cylinder was 25 inches in circumference and 13 inches long, and was turned by an electric motor (acting through various retarding mechanisms) which caused it to rotate at the rate of one revolution in nine seconds. The syllables were constructed according to the general rules for the Ebbinghaus–Müller syllables[1] but as the peculiarities of English vowels and diphthongs do not admit of an arrangement identical with that of German syllables, the construction of the English syllables underwent some unimportant modifications. The chief aim was to secure syllables which were not suggestive one of the other, and which were as little as possible liable to suggest any associations whatever. These syllables were written in vertical columns (twelve in a column), on twelve strips of paper (four columns on a strip) which were fastened as required, on the cylinder above mentioned. A screen was placed in front of the rotating cylinder. The subject sat on a chair before it, and through an opening in the screen on a level with the eyes, he read each syllable as it moved past. Before beginning with the chief series, the syllables of which were written at unequal intervals, each subject read and learned four series of syllables written at equal intervals (1.75 inches). In reading these syllables the subject invariably fell into one of the four common verse rhythms. In all the other series, the syllables were arranged at uneqnal distances from one another. Though the intervals were not the same on any two papers, a description of one paper may give an idea of all.

First Paper.

Distance between the 1st and 2nd syllables, 2.625 inches.
Distance between the 2nd and 3rd syllables, 1.75 inches.
Distance between the 3rd and 4th syllables, 0.875 inches.

These distances, representing 3, 2 and 1 units respectively, were repeated until twelve syllables had been written in a column. Here of course we have not absolute irregularity, but rather a complex rhythm. On the second paper, the intervals were again in the ratio of 3, 2, 1, but were repeated more irregularly. On the third paper, the arrangement of intervals was again still more irregular, the distances varying from .625 of an inch to 5 inches. Care was always taken to keep such a distance between the syllables, that but one syllable could be seen at one time through the screen.

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[1] Ebbinghaus: Ueber das Gedächtnis, Leipzig, 1885.
Müller u. Schumann.
Müller u. Schumann: Experimentelle Beiträge zur Untersuchung des Gedächtnisses, Leipzig, 1893.

The experiments were made with the assistance of seven competent subjects, five men and two women including the writer. With one exception, these subjects were all unacquainted with the purpose of the experiment. Before the tests were finished, each had discovered that rhythm was involved in the reading, but no one had any idea that the aim of the investigation was to determine the extent to which irregularity among intervals might exist and rhythm still be maintained. The experiments lasted 25 days for each subject, and were conducted at the same hour every day.

The plan for daily work, after the preliminary days upon which series of syllables at regular intervals were read and learned, was as follows:

First Day—Dec. 5, 1900, 8.30 A. M. First paper.

Rotation of Cylinder, 9 seconds. Controlled immediately before and after every test.

Conductor, M. K. S. Subject, Mr. A.

1. Reading first Series (S$_1$)—20 Repetitions.
2. Reading second Series (S$_2$)—20 Repetitions.

Second Day—Dec. 6, . 8.30 A. M. First paper.

Rotation of Cylinder, 9 seconds.

1. (a) Reading of S$_2$ until learned. Repetitions necessary, 25.
   (b) Test. Reproduction S$_2$ (*Treffer*). Syllables reproduced, 4.
2. (a) Reading of S$_1$ until learned. Repetitions necessary, 20.
   (b) Test. Reproduction S$_1$ (oral reproduction of whole series). Syllables reproduced, 12.

Pause 2 minutes.

Second Paper.

3. Reading of S$_3$ —20 Repetitions.
4. Reading of S$_4$ —20 Repetitions.

The work for the remaining days was similar to that for the second day. With four subjects this plan was pursued without change. With the other three subjects, the only deviation was that each person was tested for a reproduction every day directly after the reading of each new series (20 Repetitions). The 20 repetitions were divided into two groups, with a pause of 30 seconds after the first ten. This procedure had nothing to do with rhythm, but was a means for resting the reader's eyes, and was a help in learning the syllables.

At the close of each experiment every day, the results were noted, including the introspections of the subject, as well as the observations of the conductor.

The memory test known as the "*Treffer*" was conducted as follows. Through an opening in the screen, a single syllable of the series which had been learned was presented. The subject was required to mention the syllable which came next in the series. Thus six syllables were presented, as a means to the reproduction of the other six syllables of the series. The order of presentation was systematically varied, so that the student never knew what part of the series would be presented.[1] The memory tests, though systematically carried out, had, of course, nothing to do with the rhythm, except that they gave greater purpose to the reading, and directed the reader's attention away from the rhythm.

The rhythm resulting from the reading was recorded as it was judged by the conductor. A certain control of this judgment was attempted by means of separate kymograph records of the reading of each subject, but the main reliance is placed upon the subjective judgment of the conductor of the experiment. In a strict sense, therefore, the results might be supposed to be influenced by the rhythmic tendencies of both the subject and the experimenter, but the kymograms give sufficient ground for thinking that the conductor's judgments were in the main correct.[2]

The following table, covering the tests of a few days with a single subject, will give some idea of the observations made and the results with reference to rhythm.

With one exception, to be mentioned later, the experiment for which a part of the record is given below, was the least satisfactory, in regard to rhythm, of any that were made. Reading from the rotating cylinder was hard for Mr. K. The motion confused his vision, while the pronunciation of meaningless syllables was very difficult. At almost every test, the effort involved was so great as to be disagreeable. Notwithstanding these difficulties, a constant effort to make unequal intervals equal was to be observed. After some facility had been attained, rhythm always appeared. With the perception of rhythm came pleasant feeling, and a sensation of relief. The learning of the four series on one paper required for Mr. K. from 30 to

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[1] Müller u. Schumann: Beiträge zur Untersuchung des Gedächtnisses, Leipzig, 1893.

[2] These kymograms were secured in the following way: After reading one of the series, another paper with the same spacings, but containing other syllables, was put on the brass cylinder which rotated as usual. After some repetitions for practice, the subject read the syllables into a small funnel connected by rubber tubing with a tambour. The vibrations of the elastic membrane of the tambour were conveyed to a pen which inscribed the movements on a paper on a second kymograph cylinder. The resulting kymograms showed the reader's groupings of the syllables. In every case, the kymogram showed a rhythmic grouping which upon the whole agreed with the impression received by the conductor at the regular reading. Owing to a difficulty in finding suitable explosive consonants for the beginning and end of the syllables that were read into the funnel, the kymograms were not always easy to decipher in detail, which probably accounts for some at least of such differences as appeared between the conductor's record and those of the kymograph.

Subject, Mr. K. ***Rhythm Results.***

Date.	Days.	Paper.	Series.	Repetition preparation.	Repetition for reproduction.	RHYTHM.	REMARKS.
Dec. 13.	1	1	S1	20		— ⏑ | — ⏑ | — ⏑ | — ⏑ | — ⏑ | — ⏑ |	
			S2	20		— ⏑ | — ⏑ | — ⏑ | — ⏑ | — ⏑ | — ⏑ |	
Dec. 14.	2	1	S2		45		Rhythm perceptible, but so irregular that it could not be indicated.
			S1		25		
		1	S3	20			Same true of new Series 3 and 4.
			S4	20			
Dec. 17.	3	1	S4		25	⏑ — | ⏑ — | ⏑ — | ⏑ — | ⏑ — | ⏑ — |	Iambic measure clear.
			S3		25	—́ ⏑ | —́ ⏑ | —́ ⏑ | —́ ⏑ | —́ ⏑ | —́ ⏑ |	Trochaic. Subject tried to group in 3s and 4s, failed
		2	S2	20			Grouped in 3s without special accent.
			S1	20			Grouped in 4s. Rhythm not to be indicated.
Dec. 18.	4	2	S1		30	— — | ⏑ — | — | ⏑ — | — | — — | — — |	
			S2		30	— — | ⏑ — | — | ⏑ — | — | — — | — — |	
		2	S3	20			Irregular. Rhythm not to be indicated.
			S4	20			Irregular. Rhythm not to be indicated.
Dec. 19.	5	2	S4		10	— — | ⏑ — | — | ⏑ — | — | ⏑ — | — — |	Rhythm irregular but with a tendency to Iambics.
			S3		20	— — | ⏑ — | — | ⏑ — | — | ⏑ — | — — |	
		3	S1	20		— — | ⏑ ⏑ — | — — | ⏑ — | ⏑ — | — |	A strong effort to secure regularity, giving a certain rhythm.
			S2	20		— — | ⏑ ⏑ — | — — | ⏑ — | ⏑ — | — |	
Dec. 21	6	3	S2		16	— — | ⏑ ⏑ — | — — | ⏑ — | ⏑ — | — |	Iambic and anapaest clear
			S1		15	— — | ⏑ ⏑ — | — — | ⏑ — | ⏑ — | — |	
		3	S4	20		— — | ⏑ ⏑ — | — — | ⏑ — | ⏑ — | — |	Though this is the measure, the Rhythm was hardly perceptible.
			S3	20		— — | ⏑ ⏑ — | — — | ⏑ — | ⏑ — | — |	
Dec. 22.	7	3	S3		12	— — | ⏑ ⏑ — | — — — | ⏑ — | ⏑ — | — |	Somewhat iambic, but very irregular.
			S4		15	— — | ⏑ ⏑ — | — | — | ⏑ — | ⏑ — | — |	
		4	S1	20		— — | — ⏑ | — | — ⏑ | — | — ⏑ | — — |	Trochaics somewhat marked.
			S2	20		— — | — ⏑ | — | — ⏑ | — | — ⏑ | — — |	
Dec. 24.	8	4	S2		10	— — | — ⏑ | — | — ⏑ | — | — ⏑ | — — |	About the same as above. Trochaics marked.
			S1		10	— — | — ⏑ | — | — ⏑ | — | — ⏑ | — — |	
		4	S4	20		— — | — ⏑ | — | — ⏑ | — | ⏑ — | — — |	Much as before.
			S3	20		— — | — ⏑ | — | — ⏑ | — | ⏑ — | — — |	
Jan. 3	9	4	S3		15	— — | ⏑ — | — | ⏑ — | — | ⏑ — | — — |	Iambics somewhat marked.
			S4		15	— — | ⏑ — | — | ⏑ — | — | ⏑ — | — — |	
		5	S1	20		⏑ — | — ⏑ | ⏑ ⏑ — | ⏑ — | — — — |	Indefinite, but Rhythm perceptible.
			S2	20		⏑ — | — ⏑ | — ⏑ ⏑ | ⏑ — | — — — |	

60 repetitions of each series. The subject preferred to group the syllables in threes, but was seldom able to do so. Upon the whole the trochaic grouping predominated. With other subjects the same tests gave different results.

From the beginning Mr. M. secured a distinct iambic measure. To do this he was obliged to make many modifications, so that the effect was markedly different from the ordinary verse measure. Where the similarity to verse rhythm was lost, a resemblance to a dancing movement was to be observed. From 23 to 35 repetitions were necessary for the learning of a series. With one exception, this was the smallest number of repetitions made by any subject.

Mrs. K. also read in distinct iambic measure. In order to equalize the intervals, she memorized certain syllables at the first or second reading, and then brought them in as she needed them to make the measure. The resulting iambic measure was as follows:

| ◡́ —́ | ◡́ —́ | ◡́ —́ | ◡́ —́ | ◡́ —́ | ◡́ —́ |

This test indicated that the intervals between the members of a group and the intervals between the groups themselves are very different matters. If either set of intervals be equalized, the rhythm is marked. If both sets be equalized the rhythm is smoother, but not necessarily more musical. From the inequalities of the time intervals Mrs. K. inferred the inequalities of the space intervals. By reason of the constant rhythm maintained, Mrs. K. learned each series more quickly than any of the other subjects.

Another subject, Mr. T., found the reading of the same syllables very difficult. He reported a "feeling of strain" in trying to catch the passing syllables. The unequal intervals were the source of some unpleasant feeling which the subject did not trace to the right source. He thought that the number of syllables was not the same in all the series. Although he read throughout in trochaic measure, his manner of reading varied greatly. His measures were characterized by pauses and shades of accent that come under no known rules of rhythm. To him, learning the syllables was very difficult, and very unpleasant. He resented the demand on his attention.

Mr. L. found the same syllables easy. He read constantly in iambic measure. He knew nothing of the purpose of the experiments, yet he constantly talked of rhythm. He found the irregularities very amusing. The rate of rotation (9 seconds) was too slow for him, so that he made many fantastic associations.

Another subject, Mr. A., secured a distinct rhythm by a procedure different from that of any other person. He grouped in twos and threes, often securing the accent by substituting time for stress. He was always successful in securing an adaptation of sound and time to the space intervals of the series. To him, the difficulties arising from unequal intervals were entirely subjective. He believed that he voluntarily created the effects. The measure secured was agreeable, and amusing, and aided in very accurate memory work. The subject said, "The rhythm is divided into twelve unequal parts, and the task is to make those parts equal. Yet it is a time and not a space equality. A new arrangement causes a feeling of uncertainty, a slight annoyance at not being able to run at once into an old, or indeed into any rhythm. This somewhat unpleasant feeling is tempered by a curiosity as to what can be done with the rhythm. When one cannot read the new series as the last was read, uncertainty and annoyance follow. After some effort, the rhythm settles itself. Satisfaction arises and things become easy." In reading the tenth paper, the first eight repetitions gave no satisfactory rhythm. Finally, something like the following was secured | — | — | ◡ ◡ — | — | — | — ◡ ◡ | ◡ — |.

This is not a verse rhythm of course. The subject reported: "The long intervals between the first two syllables broke up my previous method of grouping. The long intervals determined one kind of foot, the short ones quite another kind. The difficulty of bringing the syllables to a satisfactory stop at the end was a source of discomfort. It was like a strain of music entirely unphrased, no cadence anywhere." The grouping varied from single syllables to anapaests, and dactyls. In the latter, time instead of stress gave the effect of accent. The single syllables were read, each in the time of the group of three. The iambic foot at the end was made merely to satisfy the ear of the subject. In the eleventh paper, the following rhythm was secured. $| - \smile | - \smile | - \smile | - | - \smile | - \smile | - |$. The objective spacings were very unequal. The whole effect of regularity and equality was secured by the voice. In the twelfth paper, the intervals varied from three-fourths of an inch to five inches, yet every series was read in trochaics. The longest intervals being at the end of the paper, the last syllables were learned by heart, and brought in at the right time. The learning of a series required from 25 to 55 repetitions.

The last set of tests in this part of the experiment was conducted by Dr. Sanford, the conductor of the former tests being now subject. On the first paper, the intervals were equal, yet, until the seventh reading, no settled rhythm appeared. Then there seemed to be a rising and falling accent upon the first and second syllables of each pair, thus, $| \overset{\diagup}{-} \overset{\diagdown}{-} | \overset{\diagup}{-} \overset{\diagdown}{-} | \overset{\diagup}{-} \overset{\diagdown}{-} | \overset{\diagup}{-} \overset{\diagdown}{-} | \overset{\diagup}{-} \overset{\diagdown}{-} | \overset{\diagup}{-} \overset{\diagdown}{-} |$. In the second paper, the measure seemed to be iambic. The syllables of the third paper seemed to fall into triple time, thus, $| \overset{\diagup}{-} \overset{\diagup}{-} \overset{\diagdown}{-} | \overset{\diagup}{-} \overset{\diagup}{-} \overset{\diagdown}{-} | \overset{\diagup}{-} \overset{\diagup}{-} \overset{\diagdown}{-} | \diagup \overset{\diagup}{-} \overset{\diagdown}{-} |$. Later, the reading seemed to approximate anapaests, or iambics. The subject reported: "The spaces between the syllables appeared to be equal, but some syllables appeared to be unduly hastened, while others were delayed. Was disturbed by the fear that I should not finish the series in time. In certain relations, some syllables are difficult to pronounce. The syllable 'war' following a difficult syllable was almost unpronounceable."

These tests constituted the main part of the investigation, and as concerns the question of a limit of inequality and irregularity of intervals in connection with voice rhythm, led to wholly negative results. *Every subject tried of his own accord to make unequal intervals equal, or at least to reduce them to some rhythmical grouping, and each succeeded. Each one strove to secure a rhythm agreeable to the ear, and, before any learning could be done, this had to be secured.*

II.

Because of the failure to obtain a definite answer to the question, Dr. Sanford proposed a series of tests in which the syllables should be presented still more "antirrhythmically," *i. e.*, the inequalities of the intervals should be greatly increased and should not recur even in the twenty repetitions of the series.

Six series of meaningless syllables (twelve syllables in a series) were arranged on a paper thirteen inches in width and forty-two feet in length. This length allowed the repetition of each series twenty times, at distances between the syllables that were never twice the same, and varied from three-fourths of an inch to ten inches. This variation extended to the different series, no two of which were arranged in the same way. The syllables, similar in construction to those used in the previous tests, were printed instead of written. Blank paper four feet in length was attached to each end of the long paper. This strip, now fifty feet long, was rolled tightly on the cylinder. The latter being set in motion, four feet of blank paper first rolled off; then the reading of separated syllables through an opening in the screen began and continued until the whole fifty feet had been run off. By the time the whole strip had been run off, one series had been read twenty times. The paper being readjusted, the next series was read in the same manner as the preceding one. Two new series were read each day, and the learning was tested as before.

The point was to find whether, with the increased and unrepeated inequalities, the subject would still read rhythmically. Of the set of subjects who served in the earlier tests, six were also employed for the new ones. In addition four new subjects were selected, who had had no experience in work of this kind.

The results will be sufficiently clear from the individual reports:

Mr. K., who had found the previous work sufficiently trying, found this confusing, annoying, and very difficult. Towards the close, some signs of rhythm were perceived. The learning was very imperfect.

As in the previous tests, Mr. M. read in distinct iambics, and learned easily.

Mrs. K., as before, read in strong iambics, and learned quickly.

Mr. S. found more difficulties than before, and could not learn until something like trochaics was secured.

Mr. A. found more difficulty than in the previous work, but after securing an anapaest movement, he learned readily.

On the first day, Miss S. had a distinct sensation of rhythm. Later she seemed to read in iambic, anapaest, or dactyllic movements.

Mr. H. (one of the new subjects) had very little sensation of rhythm, though he finally grouped in twos. He could not learn the syllables, but thought that he could have done so by singing them.

In reading the same series, Mr. H.'s son, a boy of thirteen, read in iambics at once. This subject knew nothing about the unequal intervals, but was annoyed by "something that broke the beat," so that he could "hardly get the syllables back into order." He said "it was like men stepping. The band played, and something came and broke the march. Several men kept time. They broke step suddenly." Toward the close of the tests, this boy sang the syllables in dactyls and kept time very well. The singing was quite spontaneous. This lad played his way through the tests, and enjoyed the reading greatly.

Another of the new subjects grouped the syllables in threes by beating with her finger. Her perception of rhythm was very slight.

The last of these subjects, Mr. S., swayed his shoulders to some rhythmic movement which he located in the screen. Otherwise he had a very slight perception of rhythm.

III.

In summing up the results of this investigation into the limits of the inequality and irregularity of intervals in connection with the rhythms of speech, it may be stated that each subject tried to equalize unequal intervals. Of eleven subjects, nine had a distinct impression of rhythm resulting from a greater or less equalizing of the intervals. The other two subjects had but vague impressions of rhythm.

Some of the devices for securing equality were as follows:

1. Certain syllables were memorized at the first reading. Later, these syllables were spoken before they appeared at the opening in the screen, or they were held back and spoken after they had passed. This hastening or delaying of the syllable was according to the need of the individual.

2. Manner of articulating became a factor in equalizing intervals. Quick, distinct utterance of a vowel lengthened the interval, while holding it with a glide shortened the interval.

5. Sometimes the suspension of the rhythm had the effect of equalizing the intervals. This differed distinctly from a pause between feet.

6. Beating time with hand or foot while reading was an important factor in securing and maintaining equal intervals.

7. Repetition of the series was a great aid in securing regularity and equality among intervals. It was also a means of developing an apprehension of rhythm. The equality was not determined by the shortest interval, but by some one interval to which the others could be the most easily adapted.

GENERAL OBSERVATIONS.

Excepting Mr. H., who had no theories regarding rhythm, the subjects all believed that without orderly speech movements, and approximately equal intervals, regularly succeeding one another, the learning of a series of syllables was impossible. When the apprehension of rhythm accompanied such movements, the best condition for learning existed.

Some of the subjects appreciated the importance of keeping one rhythm. Mr. M. and Mrs. K. established an iambic rhythm, and maintained it throughout their work. These two learned with much greater ease than those who showed greater variations in rhythm.

After the pleasant feeling resulting from rhythm had been once experienced, it came to be regarded as very important in learning, while the unpleasant feeling resulting from irregularity and confusion was an absolute hindrance to the same work.

Although the learning of the syllables was the chief aim of all but one of the subjects, the conscious effort was directed mainly to the equalization of the intervals. After a rhythm was established, its tendency to persist and to dominate the mental state was marked. So long as a rhythm remained unchanged, the substitution of one letter for another, at the beginning of a syllable, was almost impossible. A syllable mispronounced at the beginning of the work must remain so throughout the test. Also the rhythm of one series had a tendency to dominate the next series. A total change of intervals did not always insure a change of rhythm. This was especially shown in the work of Mr. M. and Mrs. K.

In these tests, both primitive (single) and complex rhythms were developed. As a rule the primitive rhythm when developed ran through the whole series, but sometimes, owing to the inequalities of the intervals, in the same series, a curious mingling of simple and complex rhythms might occur.

In reading with primitive rhythm, no stress is placed on the syllables. The rhythmic expression is secured by the management of the intervals between the syllables. The subject realized the monotony of this reading, and as soon as facility was secured, made an effort to avoid it. The first reaction against "level" reading was shown in a tendency to accent every syllable, a procedure only less tedious than the one it supplanted. Just here arose the necessity for grouping. The limitations of respiration made it impossible to accent more than three syllables with one breath. A strong accent on each member of a group involved still more frequent inhalations of breath. The untrained subject was obliged to take a breath at the end of every two or three syllables. These peculiarities of breathing are shown clearly on the kymograms.

The next step from a simple towards a more complex rhythm consisted in grouping, and placing an equally strong accent on each member of the group. After some repetitions, the subjects (except Mr. H.) all showed a tendency to accent some one member of a group, especially, so that the effect of verse rhythm was secured. Of seven subjects who served for the regular experiments, four grouped mostly in iambics, and three mostly in trochaics. In the extra tests, only the youngest of the new subjects read in iambic measure. Among the subjects for the regular tests, two read exclusively in iambics, and one nearly always in trochaics. The other subjects read more or less in dactyls or anapaests, though they seemed to prefer iambics or trochaics.

The exigencies of the irregular arrangement often made necessary a grouping that, though rhythmic, belonged to no one of the four-verse rhythms. At times three of the four typical measures were combined, while at other times, the reading involved one or two of the regular measures, together with groups of evenly accented syllables, and even with isolated members strongly accented. Often, while the conventional group was preserved, the rhythmic effect was dominated by the single syllables. In three cases, the rhythm was like that of a dancing movement.

The musical effect of the anapaestic and dactyllic measures was greater than that of the iambics, and trochaics. With the three-part measures, the learning of the series was no more difficult than with the two part measures, but with the former the reproduction of syllables—after an interval—was much less certain than with the latter.

SPINOZA AND MODERN PSYCHOLOGY.

By Amy E. Tanner.

Spinoza's proof *de more geometrico* and his great labors to reduce all the statements of his ethics to this form, have oftentimes blinded the student to the true modernity of his ideas. To find peering out from his stilted phraseology and labored demonstration a theory of the origin and development of the mind that is essentially modern and that forecasts much of modern method, is an interesting experience, especially when we realize that this was probably the theory which he employed unconsciously in everyday life.

With Spinoza's general theory every one is familiar. There is but one substance, God, which manifests itself under the two attributes of thought and extension. These two attributes run parallel to each other in all their modes, and hold no causal relationship to each other except as they are each referred to God.

Here at the very beginning we see an interesting obscurity in Spinoza's thought. He says distinctly that mind and extension are not different substances but the same seen under a different attribute. Is it not then quite as unsuitable to speak of a parallelism between them as of a causal relation? Ought we not rather to say that when God reveals himself as thought then this extension appears? But leaving this aside, if he admits that there is but one substance, how can he deny a causal relationship between all forms of this substance?

Taking the theory more in detail and rearranging the order in which Spinoza gives his proofs in order to show the parallelism between him and modern theory, we get the following.

In Part 3, Props. 6, 7 and 9, Spinoza states that everything endeavors to persist in its own being, and that this endeavor is its actual essence. When referred to the mind solely, the endeavor is called will; when to the mind and body in conjunction, appetite, and when the mind is conscious of its endeavor, the consciousness is desire. What would Spinoza call the endeavor when referred solely to the body?

The similarity of this to our modern idea, is very striking. We tend more and more to look upon our mind as it functions to-day as the residuum of countless efforts at self-preservation. One theory that is very commonly accepted states explicitly that mind arose in the effort of the body to preserve itself, and that when no further effort is necessary, mind retires. This again makes striving the essential feature of the mind, the matrix, so to speak, out of which are differentiated later thought and feeling.

This also is Spinoza's idea, for he goes on to say (Part 3, Props. 11 and 12) that since the essence of the mind is will or the attempt at self-preservation, we shall have pleasure in the passage to a greater perfection and pain in the passage to a lesser one, and we shall endeavor to conceive only the first. The modern pleasure-pain theory, while it realizes more fully than Spinoza could the difficulties in the way of accepting this idea unreservedly, still has no better general theory. It too, says that on the whole pleasure is the sign of well-being and pain of harmfulness to the body.

These general assumptions that Spinoza makes with regard to persistence and to pleasure and pain, make his theory essentially an evolutionary one, in spite of his efforts to make it mathematical. And yet this very endeavor to employ mathematical demonstration is one proof of his modern spirit. If, instead of geometry, he could have used such forms as now are employed both in our physical and psychological laboratories, he would have had the exactness for which he so longed without at the same time petrifying his theory. But he was too early in time for those forms of mathematics.

When we take up more in detail Spinoza's theory of knowledge, we see even more clearly both his closeness to the modern spirit and his separateness from it. He does not argue as to the origin of knowledge. He assumes it in Part 2, Axiom 3, where he says: "Modes of thinking, such as love, desire or any other of the passions, do not take place unless there be in the individual an idea of the thing loved, desired, etc.," and in Prop. 13, he says and demonstrates that "The object of the idea constituting the human mind is the body, in other words, a certain mode of extension which actually exists, and nothing else," and in Prop. 11, "The first element which constitutes the actual being of the human mind is the idea of some particular thing actually existing."

That is, the world of extension is the object without which the mind would have no ideas, that is, actually no existence. Surely this comes very close to being a sensationalistic theory of knowledge, in spite of a formal assertion which he makes later on. For all practical purposes it is both sensationalistic and causal, as one can reason from mental to bodily effects and from bodily to mental with complete accuracy. What he really is saying is that without some material with which to work the mind could have no existence, and this material must be the other attribute of God, the one substance. If God stands ready to display a certain mode of extension every time that there is a certain mode of thought, it surely is not far from being a practical cause and effect relation between the two, though it does seem rather hard on God to make him into a go-between.

Since the object of the human consciousness is only the body, then the mind (Part 2. Props. 14 *et seq.*) will perceive things in proportion as the body is capable of receiving impressions; the ideas that we have bear a direct relation to the number and delicacy of the sense-organs and to our general bodily structure. What we consider our knowledge of external nature is in reality far more a knowledge of our own bodies. To take a modern instance, if we say a book is heavy we mean that our muscles react in a certain way for which our mind has a certain name. Another person may say that the same book is light. The quality of the object is really due to the condition of our sense-organ.

This is the beginning of Berkeley's idealism, one would say, as well as of much of our modern educational and ethical theory, which sets about giving sane ideas of the world by developing a healthy body. If the content of the mind consists solely of its consciousness of bodily changes, then surely nothing is more important to mental balance than bodily health. I do not know that Spinoza elaborated this idea in any of his writings, but his life exemplified his belief in it.

Along with this goes naturally a theory of illusions and of association of ideas (Props. 17 and 18). If we have any idea of an external object, we shall consider it as actually existent, unless we can disprove its existence by means of the other senses. Memory and imagination are explained as due to the permanent changes made in the soft parts of the body by external bodies striking upon the bodily fluids, which in turn strike on these soft parts and alter them. Then if, at another time, without an external body striking the fluids, the fluids strike the soft parts, the idea of the external body will be aroused. In the same manner, if two ideas have been aroused at once, the later arousal of either tends to call up the other. If Spinoza had known a little more about the nervous system he would have written James's chapter on Association of Ideas some two hundred and fifty years ago.

In Prop. 23 we find a valuable suggestion as to psychological method. The mind cannot know itself except in so far as it perceives the ideas of the modifications of the body; that is, to know ourselves we must study perceptions and sensations primarily, not theorize abstractly on the nature of the mind. We must observe the effects of outside stimuli and trace the connections between external objects and conscious manifestations. We can imagine that Spinoza would heartily adopt experimental psychology, especially as it would afford some of the mathematical demonstration which he loved so well.

In Props. 38 and 39 of Part 2, he also hints at the reason why man is social. Some ideas are common to all men because their bodies agree in certain respects. Then the more that a man's body has in common with other bodies, whether human, animal or inanimate, the more will he have in common with them and the more adequate will the ideas be. That is, to use modern language, our brotherhood with man is due to an actual physical likeness. Or, in Spinozistic speech, each of us is but a mode of the one substance, and the more that mode has in common with other modes whether of thought or extension, the more will we know them. Sympathy rests upon a basis of like feeling, and like feeling is impossible unless the one body is like the other body.

These adequate ideas must not be considered as concepts, or what Spinoza calls general ideas. The latter arise from the fact that our imagination is unable to picture distinctly all the individuals of a class, becomes confused, and loses sight of the minor differences, thus confounding the individuals and keeping clearly in mind only the points in which they all agree. This sort of knowledge can not be depended on, while adequate ideas and intuition can be.

But when we ask how we can know a true idea, we come to what is probably the fundamental difference between Spinoza and modern thought. In Part 2, Prop. 43, he says, "He who has a true idea simultaneously knows that he has a true idea and cannot doubt of the truth of the thing perceived." Again and again he asserts this and he refuses to discuss the question of whether the idea truly represents the object, saying, "As to how a man can be sure that he has ideas that agree with their objects, I have just pointed out with abundant clearness that his knowledge arises from the simple fact that he has an idea which corresponds with its object, in other words, that truth is its own standard."

The centre of his proof seems to be his assumption that we are necessarily conscious of any idea that we have, that is, that consciousness and self-consciousnees are really the same, or that if we have an idea we must know that we have it, and not only that, but must know all its characteristics. Then we ought to know not only true ideas at sight, but false ones, and never ought to be deceived. Spinoza is not at all satisfactory here. He says that a false idea is due to confused, fragmentary or inadequate ideas, but still does not explain why we do not perceive this confusion, etc., if the idea carries its own sign with it.

In his theory of the will he is once more very modern. It is not in the nature of reason to regard things as contingent, but as necessary. The mind is always determined by a cause, which is determined by another cause, and so on *ad infinitum*. There is no freedom of the will.

Still more, both will and understanding are only abstract terms for a sum of particulars. There is no will in the abstract apart from particular volitions, and there is no particular volition apart from ideas. Will and understanding are one and the same thing. There can be no volition or negation save that which the idea, by the very fact of its being an idea, involves (Part 2, Props. 48 and 49). An idea is not like an inanimate picture on a panel he says in various places. It is a living, growing thing, which by its very nature involves a judgment, an affirmation, that is, a volition.

We could hardly have James's theory that all consciousness is motor, stated more explicitly in the absence of modern proofs of it. The denial that mind and volition have an abstract existence, an existence apart from thinking and willing, and the assertion that the essence of the mind is will and that understanding and will are identical;—these assertions take Spinoza over into the most modern of modern thought. The inner need for exact proof must indeed have been compelling in order to force such ideas into the strait-jacket of geometrical demonstration.

ON THE NAMING OF COLORS.

By H. E. Houston and W. W. Washburn.

The ancients, as is well known, had comparatively few color names and used these loosely. This deficiency does not necessarily mean an inability to discriminate colors, nor does an increase in color vocabulary mean an increase in the nicety of discrimination. The completeness of the color vocabulary depends rather on the extent of the social needs of designation. The color names of savage and semi-civilized peoples of the present day have been found to be quite defective, though their powers of discrimination are exceedingly fine.

Our own system of color names and their application leave yet a good deal to be desired. Efforts have been made at various times to introduce arbitrary standards, but none has been widely accepted, except the designation of the standard hues by the corresponding wave-lengths in the spectrum or by reference to the Fraunhofer lines. For the purples, grays, and the multitude of tints and shades this, of course, makes no provision.

On the other side of the question—the precision with which color names are now actually applied—a few observations have been made and at least one experimental study has been carried out.

Prof. H. K. Wolfe[1] made an investigation of the color vocabulary of school children. The children were asked to name colors which were shown to them. The colors used were oil pigments applied by means of a brush to cardboard, previously treated with a coating of glue. Comparison was made on the basis of age and not of class standing. The answers of those deficient in color vision were classified by themselves. The results were then tabulated, giving the correct answers in 1,000. Males and females were classified separately. Colors are named in an entirely different order from that given by Preyer. Tables are given showing the rate and time of improvement, also sex differences. Many colors are absurdly named, and the conclusion is that the color instruction has been very defective.

Our own experiments were in general method akin to those of Wolfe, but carried out upon subjects of a greater range in age. The colors selected for these sets of tests were the twenty-four spectrum colors of the Prang Educational Company. The purpose of the experiments was to see how the colors would be named by school children and college students if left to themselves and not aided by any artificial system. The colors were cut in squares twenty centimeters on the side with a border of black of two centimeters in width. These colors were held up before the various classes in a good light, direct sunlight being avoided. The pupils observed the colors and wrote their answers independently on paper. In the lower primary grades where the children were unable to write they were questioned independently, out of the hearing of the other pupils. In other respects the tests were exactly the same as in the other grades. The colors were purposely arranged in the order of every sixth, *i. e.*, if we represent the colors of the Prang system by the numerals 1, 2, 3, ... 24, the order in which the colors were presented would be 1, 7, 13, 19; 2, 8, 14, 20, etc., . . . 24. The colors were each exhibited for about one second. A considerable number of names were given, including the names given to various colored fabrics, the colors of flowers, of plants and of concrete objects, such as brick-red, etc., but all of these together form a very small percentage of the whole number of answers. It was, therefore, thought best to classify these under the headings of red, yellow, orange, green, blue, and purple.

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[1] H. K. Wolfe: Studies from the University of Nebraska, 1890.

The first of the following tables gives the measurements of the Prang colors in terms of the five color standards used in the Physical Laboratory at Columbia University. Thus 90° of the first red of the Prang series plus 10% of white matches 57% of the Columbia red, plus 6.7% of the Columbia blue, plus 36.3% of black.

The later tables give the percentages of answers for each of the colors for the kindergarten pupils and for the whole number of subjects tested.[1] The accompanying curves give the latter results in graphic form, the ordinates denoting the percentage of answers for each Prang color and the abscissæ denoting the position of the same color in the spectral color system. This position was determined by estimating the distance between the successive steps in the following way:—A large circle was drawn on which were placed the twenty-four colors in their regular order, from 1 to 24. Then the distances were altered until they seemed to represent the proper differences in color sensation. The estimates were made by seven subjects and the abscissæ used are the averages of the seven estimates.

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[1] The percentages for the kindergarten year only are here given separately as there are no striking differences among the other years.

Measurements of Prang Colors. Columbia Standards.

Prang Colors.	*R.*	*O.*	*Y.*	*Y.*	*B.*	*BK.*	*W.*
90 R 10 W	57.0	......	......	......	6.7	36.3	......
96 R R O 4 W	100.0	......	......	......	......	......	......
98 R O 2 W	86.4	13.6	......	......	......	......	......
100 O R O	50.0	50.0	......	......	......	......	......
100 O	......	71.8	8.4	......	......	14.4	5.4
100 O Y O	......	61.7	26.3	......	......	6.7	5.3
100 Y O	......	40.5	42.5	......	......	10.2	6.8
100 Y Y O	......	18.7	77.3	......	......	......	4.0
94 Y + 6 BK	......	......	81.2	18.8	......	......	......
84 Y Y G + 16 BK	......	......	47.0	53.0	......	......	......
98 Y G + 2 W	......	......	28.3	71.5	......	0.2	......
100 G Y G	......	......	11.6	56.6	......	31.8	......
100 Y	(R	.644)	2.1	39.9	......	58.0	......
100 G B Y	(O	.614)	......	27.7	-9.6	62.7	......
100 B G	(Y	.585)	......	29.4	28.2	42.4	......
100 B B G	(G	.518)	......	23.6	67.2	8.8	0.4
62 B + 38 BK	(V	.452)	......	-4.8	88.1	......	7.1
100 B B V	......	......	......	2.4	92.7	2.9	2.0
100 B V	1.9	......	......	......	56.6	41.5	......
100 V B V	4.3	......	......	......	45.2	50.5	......
100 V	7.1	......	......	......	38.6	54.3	......
99 V R V	12.0	......	......	......	14.1	73.9	......
96 R V + 4 W	19.8	......	......	......	8.8	71.4	......
90 R R V + 10 W	38.7	......	......	......	4.7	56.6	......

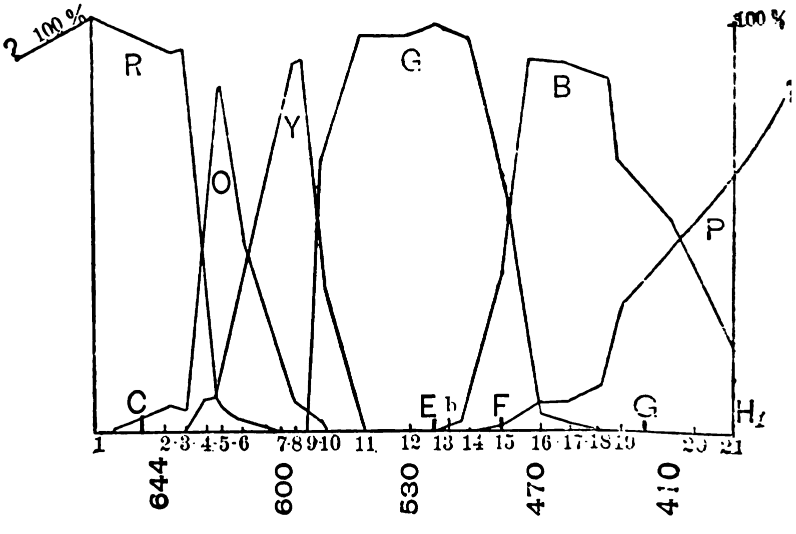
No. of Experiments, 48–53. Kindergarten Pupils. Av. Age 5.

No.	Name.	Red.	Orange.	Yellow.	Green.	Blue.	Purple.
1.	R	100	—				
2.	RRO	97.8	2.2				
3.	RO	95.7	4.3				
4.	ORO	37.0	48.2	12.8			
5.	O	14.0	50.0	36.0			
6.	OYO	5.0	57.4	37.6			
7.	YO	3.0	31.0	66.0			
8.	YYO		12.0	88.0			
9.	Y		11.0	84.0	5.0		
10.	YYG			40.5	51.0	8.5	
11.	YG			5.5	96.0	8.5	
12.	GYG			3.3	89.2	7.5	
13.	G			3.3	84.9	11.8	
14.	GBG				88.0	12.0	
15.	BG		*Miscellaneous.*		67.2	32.8	
16.	BBG		5.5		11.0	83.5	
17.	B		6.0		3.5	90.5	
18.	BBY		6.5		1.5	90.5	1.5
19.	BY					94.0	6.0
20.	YBY					70.5	29.5
21.	Y	8.0				54.0	38.0
22.	YRY	21.6				31.8	40.3
23.	RY	74.4				12.8	12.8
24.	RRY	87.2					12.8

Averages of all Experiments.

No. of experiments, 1,573. **Ages, 5 to 24 years.**

No.	Name.	Red.	Orange.	Yellow.	Green.	Blue.	Purple.
1.	R	97.1	0.9				
2.	RRO	93.9	6.1				
3.	RO	96.8	3.2				
4.	ORO	19.2	80.8				
5.	O	4.4	95.6				
6.	OYO	0.8	41.1	58.1			
7.	YO		25.2	74.8			
8.	YYO		8.3	91.8			
9.	Y		1.9	98.1			
10.	YYG			23.6	76.4		
11.	YG			3.8	96.2		
12.	GYG			2.9	97.1		
13.	G				99.8	0.2	
14.	GBG				97.2	2.8	
15.	BG		*Miscellaneous.*		58.1	41.9	
16.	BBG		0.4		2.4	97.2	
17.	B		0.5			93.0	6.5
18.	BBV		0.6			84.2	15.2
19.	BV					69.2	30.8
20.	VBV					66.1	
21.	V	5.3				31.7	63.0
22.	VRV	29.4					70.6
23.	RV	92.6					7.4
24.	RRV	98.1					1.9



As points of special interest in the curves may be noticed the somewhat broad range of colors that were almost always denominated "red," "green" or "blue." For "orange" and "yellow" the total range is perhaps nearly as great, but the high frequency of usage is confined within extremely narrow limits. Another item is the sharpness of the demarkation in the usage of "green" on the side toward red as compared with its wide range on the side toward blue, corresponding to the common uncertainty of "telling green from blue." In a somewhat similar fashion "blue" and "purple" overlap. It is also interesting to notice that the pair "blue" and "yellow" do not overlap at all, though they might perhaps be expected to do so, if it were actually possible to see in green a mixture of yellow and blue as some have thought could be done. A similar, though smaller, gap separates the usage of the names "red" and "green." "Green" and "purple" (here equivalent to violet probably) were, however, applied to identical colors by different individuals.

PSYCHOLOGICAL LITERATURE.

The Nature of Truth; an Essay, by H. H. JOACHIM. Oxford, Clarendon Press, 1906. pp. 182.

Pragmatism, a New Name for Some Old Ways of Thinking; popular lectures on philosophy, by W. JAMES. New York, Longmans, Green & Co., 1907. pp. xiii, 309.

These two works lie too far beyond the confines of psychology to receive detailed treatment in a psychological journal. They offer, however, an excellent illustration of that fundamental difference of temperament which Professor James lays at the basis of all philosophical differences; and they remind us that the psychology of temperament is so far in its earliest infancy.

Mr. Joachim dismisses pragmatism, in his preface, as unworthy of serious consideration. "In substance the doctrine remains what Plato proved it to be: not a new theory of truth, but a denial of truth altogether." His own object is to "examine certain typical notions of truth, one or other of which . . . has hitherto served as the basis of philosophical speculation." The first of these regards truth as correspondence; the second, as a quality of entities entirely independent of mind; and the third as coherence. These are discussed in as many consecutive chapters, and the essay concludes with a fourth chapter on the negative element and error. The upshot is that "the coherence notion fails of complete success; but it has carried us further into the heart of the problem than either of the other two notions, and it has maintained itself against difficulties to which they succumbed."

Professor James—who does not fail to signalize the mainly negative outcome of Mr. Joachim's essay as, in so far, an argument in his own behalf—presents pragmatism, in eight Lowell lectures, as essentially a mediator, methodologically and metaphysically, between absolute idealism and empiricism. The lectures deal with the present dilemma in philosophy—namely, the antithesis just mentioned; the meaning of pragmatism as method; the pragmatic attitude to some metaphysical problems, such as substance, God, free will, design, the one and the many; the relation of pragmatism to common sense; pragmatism's conception of truth; pragmatism and humanism; pragmatism and religion. The book makes delightful reading; but it does not appear that the fundamental confusion often charged to the account of the pragmatist, the confusion of truth with knowledge of truth, is finally cleared up. And it may be doubted whether the middle position finally recommended will satisfy many temperaments, since a temperament, however mixed its origin and character, generally leans with some bias to the one philosophical side or to the other. To put the matter crudely, Professor James will probably be too religious for the non-religious, and not religious enough for the religiously minded.

P. E. WINTER.

Essay on the Creative Imagination, by T. RIBOT. Translated from the French by A. H. N. Baron. Chicago, Open Court Publishing Co., 1906. pp. xix, 370.

M. Ribot holds an unique position among descriptive psychologists.

Every few years he calls attention, in an elaborate essay, to some field of work—general ideas, affective memory, creative imagination, the passions—that has so far been neglected, whether by the descriptive or by the experimental school. The combination of a strong systematic bent with a high degree of literary skill makes this series of books doubly valuable; they are found readable by the general educated public, and they offer to the psychologist, without pretence of finality, a conspectus of fact and opinion gleaned from scattered sources and arranged on a workable plan. Mr. Baron's translation is, therefore, most welcome; we may safely predict that one of its results will be the carrying of various part-problems of imagination into the laboratories. The translation itself, while by no means sympathetic, appears to be accurate. The proof-reading of the book has been very carelessly done. P. E. WINTER.

Text Book of Psychiatry, a psychological study of insanity, by DR. E. MENDEL. Authorized Translation. Edited and enlarged by Wm. C. Krauss. F. A. Davis, Philadelphia, 1907. pp. 311.

The psychiatrical clinic is now obligatory to medical students in most countries and schools, and in Berlin since 1901 has been a topic on which all doctors must be examined. There are of course very many excellent texts on the subject in German, but the author here tries to present it in a general perspective to enable students to fill up the gaps in the clinic and to reduce all to its proper time and place. The author himself stands in the front rank of German men of science and for thirty years has been a leader in Germany and also distinguished by his hospitality to Americans. The plan of the book represents Mendel's latest views of classification. Under general symptomatology he discusses disturbances of sensation, of thought, memory, feeling, judgment, conscious action, including speech, and has a pregnant chapter on physiological disturbances in the condition of the body, including physical degeneracy, motility, reflections, basal motor nerves and internal organs. Under etiology, a chapter on statistics discusses the factors favoring breaking out of disease, such as puberty, climacteric and old age. Direct causes are classified as psychic, somatic and mixed. Outbreaks may be transitory, acute, or chronic. There are sections on pathological anatomy, diagnosis, prognosis and treatment. Then he discusses special psychiatry, imbecility, idiocy and psychosis, hallucination, mania, melancholia, circular psychosis, acute dementia, psychosis from central neuroses such as epilepsy, hysteria and chorea, with a special section on intoxication. Then follows an account of the psychosis due to thyroid trouble, to poisons from without, to organic and inorganic poisons. Under organic psychosis he discusses diffused diseases of the cortex including progressive paralysis, senile dementia, arterial sclerotic psychosis and apoplexy. The work ends with instructions for examining a person mentally diseased and for rendering an opinion.

Psychology Applied to Medicine, an introductory study, by DAVID W. WELLS. Davis, Philadelphia, 1907. pp. 141.

The author thinks the proposition "*all* disease is mental" seems so absurd to the medically trained man that he is apt to ignore the fact that some disease is mental. He advocates psycho-therapeutics, including hypnotism in special cases. Indeed, this mode of cure began with Edward the Confessor, in the eleventh century, who cured the king's evil. The author repudiates Mrs. Eddy, metaphysical healers and theosophists, agrees with Wetterstrand that there is no better cure for insomnia than hypnosis and that special soporifics are injurious. He believes that sometimes hypnosis helps neuralgia, stammering and the cigarette habit. Physicians like J. J. Putnam who is treating hysteria by Freud's psycho-analytic method, Quackenbos of New York, Dubois of Berne, the author of the very notable "Psychic Treatment of Nervous Disorders," and the general use of placebos, the growing view that drug therapeutics, which requires only the writing of prescriptions as the easiest thing, often leads directly to patent medicines, the patient seeking to avoid the middleman and get the goods direct (the physician being only a middleman for the drug maker): all these show the tendency which this book also exemplifies. The writer believes that the psychic element is present in all therapeutics, in surgery, electro-therapy, massage, that in quack medicines it is often not a drug but this element that cures, and that the same is true to some extent with prescriptions of regular physicians. We must not shut our eyes to the real value of this element because it has been abused by Christian Science.

The Influence of the Mind upon the Body, by PAUL DUBOIS. Translated from the fifth French edition by L. B. Gallatin. Funk & Wagnalls Company, New York, 1906. pp. 63.

The dependence of the soul upon the body commences in the cradle and finishes only with the grave. This volume does not deal with the speculative or the philosophical aspects of the subject, but rather with those that are moral and therapeutic. The object of culture is to subdue the body till it becomes a perfect servant of the soul. Self-control begins in the domination of the body by the soul. There are no imaginary sick people, but all suffer and therefore all are worthy of compassion and in need of cure. Exaggerated emotionalism is one of the most common diseases of modern life and nothing is more contagious than nervous affections. Many of these arise under the influence of real, and others from an imaginary, fatigue. The best cure is in the domain of higher morality, which does have remarkable power to steady the whole personality. We must have some kind of religion and philosophy and live up to it, and our philosophy must include some method or cure of anxiety.

A Treatise on the Motor Apparatus of the Eyes, embracing an exposition of the Anomalies of the Ocular Adjustments and their Treatment, with the Anatomy and Physiology of the Muscles and their Accessories, by GEORGE T. STEVENS, M. D., Ph. D. Philadelphia, F. A. Davis Co., 1906. pp. xiv, 496; 184 illustrations, some in colors and many original.

This work, by one of the leaders among American ophthalmologists, is in reality a sequel to the same author's earlier work on Functional Nervous Diseases, published in 1884, in which was first emphasized the causal connection of motor anomalies of the eyes with many functional nervous troubles. The present work gathers up the results of many papers published in the intervening years in the *Archives of Ophthalmology* and the *Annals d' Oculistique*, and gives them here again in connection with brief statements of current knowledge with reference to the anatomy of the eye muscles and adjacent parts, and with reference to paralytic and obstructive troubles of the same organs.

The work opens with an historical sketch of strabismus and heterophoria. Part I follows with nearly sixty pages upon structural relations, including a section on comparative anatomy. Part II deals in about twice as many pages with the physiology of the eye muscles and their functions. The longest section is Part III, devoted to anomalous conditions of the eye-muscles not dependent upon disease. It is these which are the fruitful source of nervous troubles.

Part IV treats in forty-five pages of conditions depending directly on disease affecting the motor functions, paralysis, spasm, etc.

While items of psychological interest will be found in other sections, it is in the second that the psychologist will come upon familiar captions like "The Visual Perception of Space," "Unconscious Conclusions," "Of Corresponding Points," and "The Horopter." And here he will find a good deal that is the result of fresh and first-hand observation, particularly with reference to physiological questions, but much also of Helmholtzian psychologizing which, by present standards, may be justly described as antique. The section is, however, one which no psychologist dealing with the phenomena of space perception will do well to neglect altogether—in especial a couple of pages of friendly criticism upon recent psychological studies of eye movements in space perception. E. C. S.

Osservazioni cliniche ed anatomiche sulle demenze post-apoplettiche. DOTT. G. MINGAZZINI, Rivista Sperimentale di Freniatria, 1897, Vol. 23, p. 821.

A point worthy of special note is the extreme emotivity of some of the patients which results in spasmodic weeping and laughing. The physio-anatomical reason of such disturbances has been referred by Brussoud to the involvement of some special classes of nerve fibres. According to him, along the anterior segment and the lance of the internal capsule, there run little bundles of cortical fibres, which extend to the centres of co-ordination of the mimetic muscles, situated in the *thalamus*. If a lesion is situated simply in contact with the anterior segment, it would have the effect of irritating the fibres which pass there; if, on the contrary, it involved the segment itself, the effect would be of a paralytic nature; in both cases the cortex would lose the power of regulating the mimetic centres situated in the *thalamus* and these would respond to all peripheral impressions, hence there would arise, sometimes spasmodic laughing, sometimes spasmodic weeping, which the patient would be unable to control.

"I have observed a true case of spasmodic weeping in two patients (Antonini and Valeri) who, whenever they attempted to reply to any question, broke into uncontrollable weeping. Sometimes a stimulus of such a nature, as to be entirely independent of every sad thought was sufficient to induce the weeping. In the first case (Antonini), there existed bilateral lesions of the lenticular nucleus which on the right side was situated near the anterior segment of the internal capsule. In the second patient (Valeri) was found on the left side a lesion of the *putamen* and of the internal membrane of the *globus pallidus*." THEODATE L. SMITH.

Hygiene of Nerves and Mind in Health and Disease. By A. FOREL. Authorized translation from the second German edition by H. A. Aikins. New York, G. P. Putnam's Sons, 1907. pp. x, 343.

M. Forel's work is a compendium of psychology, physiology, pathology and hygiene, dominated and unified by the author's well-known monistic philosophy. It is written with all the reformer's enthusiasm, which carries the reader triumphantly over a mass of details that, otherwise presented, would be dry enough. The defect of this quality is, of course, a one-sidedness that makes the book distinctly irritating to the specialist. There can, however, be no doubt that its general influence will be wholesome.

The translation is very uneven. Alongside of much careful work we find slips of grammar ("a dozen children of the same parents deviate from each other"), disturbing colloquialisms ("precious little can be gotten out of the words Nature and Natural"), uncertainties of terminology ("psychopathias" and "psychopathies"), awkward phrases ("a fish running away at our approach"), etc., etc. There seems, too, to be no clear principle running through the notes added by the translator. If the reader needs to be told that "the word Phenomenon as used in psychology or any other science does not mean something remarkable, but merely something that can be observed" (p. 17), it is surely superfluous to warn him that the Spinoza-Fechner law of identity is "to be distinguished from the logical law with the same name" (p. 78)!

P. E. Winter.

Morris's Human Anatomy: a complete systematic treatise by English and American authors. Edited by H. Long and J. P. McMurrich. Fourth edition, part iii: The Nervous System; Organs of Special Sense. Philadelphia, P. Blakiston's Son & Co., 1907. pp. x, 747-1113.

The psychologist is more likely to consult a work on physiology or histology than a text-book in anatomy. Nevertheless, anatomy must, at times, be referred to; and on such occasions this present volume of the revised Morris will prove of great assistance. The part deals with the nervous system, eye, ear, tongue and nose. The illustrations are numerous, clear (many of them are partially colored), and well-chosen, and the text is up to date. The section on the nervous system has been corrected and largely rewritten by Professor Hardesty, of the University of California; that on the eye by Dr. Gunn, of the London Ophthalmic Hospital; and that on ear, nose, and tongue by Professor Kerr of the Cornell Medical College. The price, $1.50, is extremely moderate.

H. E. Hotchkiss.

BOOK NOTES.

Laboratory Manual of Psychology, by Charles Hubbard Judd. Volume II of a series of text-books designed to introduce the student to the methods and practices of scientific psychology. Charles Scribner's Sons, New York, 1907. pp. 127.

The chapters are on the quantitative study of geometrical optical illusions, characteristics of the different parts of the retinal field, color sensations, monocular visual experiences, binocular visual experiences, binaural recognition of direction, tonal sensations, cutaneous sensations, tactual space perception, sensation intensities, apparatus and methods for recording movements, changes in circulation accompanying changes in consciousness, changes in muscular tension of the voluntary muscles, muscular co-ordination, unnoticed variations in simple co-ordinated movements, voluntary modifications in movement, analysis of voluntary co-ordinations, effects of practice (a) impression factors, effects of practice (b) motor and perceptual habits, distraction and fatigue, memory, fluctuations of attention, scope of attention and consciousness, æsthetic appreciation, experimentation with complex mental processes.

Woman and the Race, by Gordon Hart. Ariel Press, Westwood, Mass., 1907. pp. 264.

The themes in this book are: Innocence versus ignorance, flower babies, woman's place in the social scheme, motherhood a joy, real paternity, a perfect body, king mind, the rationale of celibacy, marriage actual and ideal, the joy of life. These very chapter heads suggest to an unusual degree the point of view and even the content of the book. It is thoroughly well meant and there seems to be nothing bad or objectionable. It might be described as excellent table talk. It makes little claim to be scientific and does not utilize the results of recent literature.

Zeitschrift für Religionspsychologie, Grenzfragen der Theologie und Medezin. Carl Marhold, Halle a. S., 1907, Band I, Heft 4. pp. 145-184.

We regret to observe that Dr. Vorbrodt has withdrawn from the editorship of the *Zeitschrift*. We are glad, however, to note that the editors hope soon to have another theologian in his place. The character of the fourth number is somewhat medical and the chief articles are the psychiatry and the care of the soul in corrective and penal institutions for women, social and individual care of the soul, ethical values under the influence of fatigue, the antipathy of the ancient Jews against pictures and images. The review department is restricted to the discussion of a single article.

L'Égalité, par J. J. ESCAUDE. Bibliothèque Coopérative Laique et Républicaine, Paris, 1907. pp. 73.

This writer discusses the idea of individuality and equality and traces the latter through the historic period, points out the chief traits of genuine equality, its sources, its relations to wealth and its future. The book is interesting and suggestive.

Die Beeinflussung der Sinnesfunktionen durch geringe Alkoholmengen, I Teil, von WILHELM SPECHT. Wilhelm Engelmann, Leipzig, 1907. pp. 115.

Diseases of the Rectum: Their consequences and non-surgical treatment. By W. C. BRINKERHOFF. Orban Publishing Company, Chicago, 1907. pp. 207.

Paranoia, its varieties and medico-legal importance, by CHARLES W. BURR. Reprinted from the Journal of the American Medical Association, June 1, 1907, Vol. 48, pp. 1852-1855. Press of the American Medical Association, Chicago, 1907.

A Case of the Loss of Memory, by CHARLES W. BURR. Reprinted from American Journal of Insanity, Vol. 63, No. 3, January, 1907, pp. 377-383.

NOTE.

Wilhelm Stern has changed his well known *Aussage* to the *Zeitschrift für angewandte Psychologie und psychologische Sammelforschung*, of which the first double number of the first volume, containing 187 pages, is at hand. Although the scope is greatly enlarged and Otto Lipmann becomes joint editor, the new *Zeitschrift* is called a broadened continuation of the old. The first number is very attractive and promising.

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